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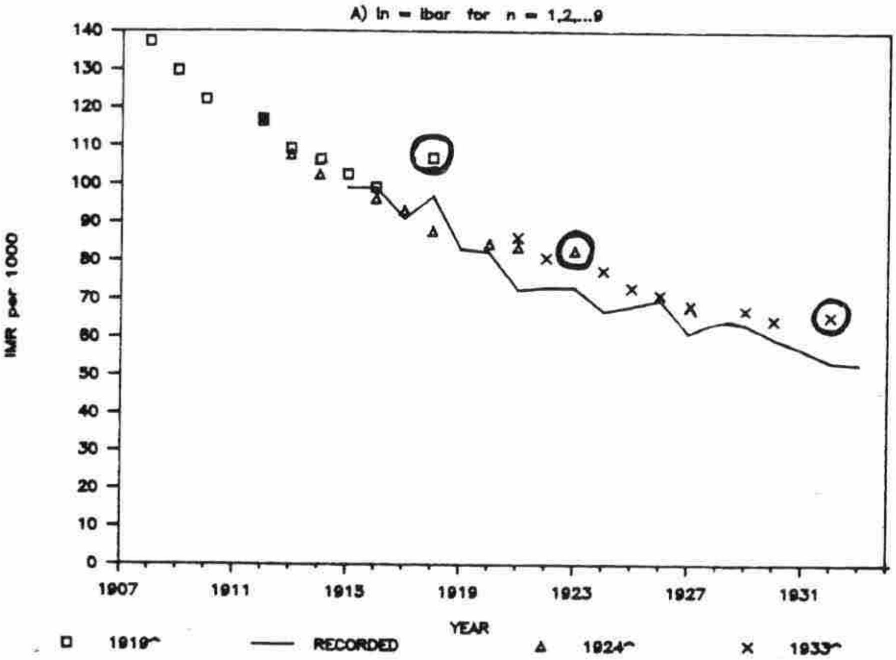
ESTIMATING CHILD MORTALITY FROM RETROSPECTIVE REPORTS BY MOTHERS AT TIME OF A NEW BIRTH : THE CASE OF THE EMIS SURVEYS

Cheikh S. M. MBACKE, Ph. D.

FIG 4.2: Estimated vs Recorded IMR for Whites in BRA



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DEMOGRAPHY

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Supervisor of Dissertation

[signature]
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Cheikh Mbackè

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ABSTRACT

ESTIMATING CHILD MORTALITY FROM RETROSPECTIVE REPORTS BY MOTHERS AT TIME OF A NEW BIRTH (THE CASE OF THE EMIS SURVEYS)

Cheikh S.M. Mbacké

Samuel H. Preston

The objective of this research is to develop a methodology for deriving child mortality estimates from the retrospective child survivorship data collected through the EMIS surveys (Infant Mortality Surveys in the Sahel). The EMIS data are of an unusual type for demographic analysis, although such data are prevalent in Africa. Retrospective studies of fertility and mortality are usually based on reports from unconditional random samples of women e.g. a cross-section of the female population at a given time. The EMIS samples are of a different type: all women are interviewed at the time of birth of a child. This type of sample is referred to as conditional (inclusion is conditioned by delivery within the reference period). The research presents a methodology for estimating levels and trends in child mortality applicable to data from conditional samples.

The methodology is tested against U.S. historical data and yields infant mortality trends similar to those recorded by the Civil Registration System. Application to the

Bobodioulasso data suggests that the infant mortality rate in Bobodioulasso in 1980 is similar to the one experienced by the U.S. White population in the Birth Registration Area by the end of World War I (about 96 per 1000 live births).

Pregnancy histories are routinely collected by some maternity clinics in Sub-Saharan Africa. Bobodioulasso maternity clinic data are shown to be of comparable quality with EMIS data. These data have not been used often to study mortality, partly because the techniques for doing so simply did not exist until very recently. One practical implication of the research is that demographers should increasingly use the facilities of the Health System for the study of mortality in developing countries in general, and in Sub-Saharan Africa in particular, now that the techniques for estimating child mortality from conditional samples exist.

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CHAPTER 1

INTRODUCTION.

1.1 Objectives.

The level of mortality during the first few years of life is probably one of the best expressions of the health and nutritional status, and, in general, of the level of economic development of a country. Changes in the well being of populations are by necessity reflected in trends of the Infant Mortality Rate (IMR), or the life expectancy at birth for example.

The twentieth century has been the scene of a spectacular improvement in longevity for the human population in general. Preston (1976) estimates that over two thirds of the gains from prehistoric times were achieved since 1900. However, when the analyst considers individual countries, it becomes clear that these survival gains have been extremely unequally distributed. As a consequence mortality differentials between countries are very accentuated, possibly more accentuated than differentials between social classes within the same country. The infant mortality rate in Bobodioulasso in 1980 is similar to the one experienced by the US-white population by the end of the First World War - about 96 per 1000 live births (see Chapters 4 and 5).

The existing evidence indicates that levels of mortality are higher in Subsaharan Africa than in any other major region of the world today (U.N., 1982). As far as trends are concerned such a clear-cut conclusion cannot be advanced. The need for more and better quality data for this region is expressed by the joint work of the U.N. and the World Health Organization in the following terms: "little is known about the dimensions of trends and variations in African mortality; this is particularly disappointing in view of the need for such information in formulating programmes to reduce prevailing levels" (U.N., 1982:83).

Even the knowledge about current mortality levels is in many instances very sketchy, not to say unreliable, because of the low quality of the data and uncertainty in the demographic techniques used to produce the estimates. Depending on the source one consults the infant mortality for much of Burkina Faso[1] in 1960-61 may be 179 or 263 (Brass, 1968), 182 or 243 (INSD, 1981) or 235 (U.N., 1985). The problem becomes more obvious when one knows that all these estimates use data from the same survey. The major difference lies in the fact that some are <u>direct</u> and based on reports on deaths in the preceding year or <u>indirect</u> because based on reports about the survivorship of children ever born.

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[1] Previously Upper Volta

Quite recently, new advances in indirect estimation techniques and not so new insights in the realm of data collection converged to give new hopes for a better quantification of mortality levels in Subsaharan Africa and in the developing world in general. The idea of collecting information on previous births at the time of registration of a current birth was expressed by Jain 21 years ago (Jain, 1965). Some maternity clinics in developing countries have included in their routine questions to women who come to seek services information on the number of previous pregnancies and live births either surviving or deceased. Chapter 2 of this study uses data of this kind collected in the maternity clinics of Bobodioulasso. El Badry (1967) used similar data to estimate fertility differentials in Bombay. Use of this type of data for the study of mortality is not widespread, because the techniques for deriving mortality estimates from them simply did not exist until very recently. Brass (1971) first suggested a way of time using such data to estimate mortality in the Soloman Islands, but his approach was later considered by himself and McCrae as unsatisfactory (Brass and McCrae, 1985).

The EMIS Surveys (Infant Mortality Surveys in the Sahel) conducted by Institut du Sahel produced this very type of data for different localities in the Sahel (See description of the surveys in Section 1.2.). The retrospective data made available by the EMIS is only a by-product of these surveys: the major emphasis was on the follow-up aspect. However, since the first application of the methodology of these surveys in 1978, it has become clear that in many instances, because of the difficulty involved by the construction and follow-up of the sample of new born, the follow-up portion might not even be able to yield one single reliable mortality estimate (see Brouard, 1985). The growing interest in the retrospective data appears therefore natural: if we succeed in deriving retrospective mortality estimates, these may serve as a basis for evaluating the follow-up estimates and vice versa. This interest was spurred by the publication of "childhood mortality estimates from non-random data (using maternity histories collected at birth registration)" McCrae 1982). The reason for the regained interest is that the article proposed an adaptation of the traditional Brass technique (Brass, 1968) to this kind of data. The need for using the EMIS retrospective data was broadly discussed in the "Seminar on the Analysis of the EMIS" held in Bamako in August, 1984 (Mbacke 1984; USED, 1984).

At its origins, which can be traced back to 1982, the major objective of this research was to develop a methodology for deriving mortality estimates from the retrospective data collected through the EMIS Surveys. Since then, advances in indirect estimation techniques have been extremely fast. Between November 1984 and December

1985, three articles were published each of them presented a different technique for mortality estimation based on data from conditional samples(Brass and McCrae, 1984; 1985; Fargues, 1985a). These techniques are brand new and have not been widely used. Their testing on an additional data set is therefore a necessary step of this research. Our aim is less to estimate levels and trends of child mortality in Bobodioulasso than to develop a methodology allowing to take best part of the EMIS retrospective data for the quantification of child mortality and to compare this methodology to alternative methodologies available. The Bobodioulasso data is used mainly because it was the first data set to be ready and available to us.

The study is divided into six chapters. The remainder of this chapter describes the EMIS Surveys. The importance of using the retrospective data of these surveys is also explained and general problems relating to the analysis of such data sketched. Chapter 2 studies the reliability of the Bobodioulasso reports on previous births. The survey reports are compared with hospital records for the sample of women for whom the matching of the two sources was possible. A critical evaluation of the existing techniques is done in chapter 3. Chapter 4 presents the theory behind the new approach advocated by this research and tests it against US data for 1919, 1924 and 1933. The new approach is used to estimate infant mortality trends in Bobodioulasso in Chapter

5 and Chapter 6 summarizes the findings and expresses a few recommendations for future research.

1.2. The EMIS Surveys

Reacting to the need for more reliable data on infant and childhood mortality in West Africa, Institut Du Sahel, since 1981, has sponsored a series of surveys called the EMIS (Infant Mortality Surveys in the Sahel). Thanks to these surveys an important mass of data has been gathered in five urban and one rural settings in the Sahelian region. The urban surveys were conducted in Banfora, Bobodioulasso, Koudougou and Ouahigoua (Burkina Faso), and in Bamako (Mali); the rural one in the Thi s Region in Senegal. The EMIS surveys have a major feature in common: their methodology. Developed by IFORD researchers in the late seventies, this methodology consists of choosing a sample of the yearly births occurring in the area under study and following them through the first two years of life. (See Houehougbe, 1981 for a detailed presentation of the methodology.) The approach was first tested in Yaounde (Cameroon) between January 1978 and December 1980; and afterwards used in Brazzaville (Popular Republic of Congo), Lome (Togo), Cotonou (Benin), Bangui (Central African Republic) and Ouagadougou (Burkina Faso).

The methodology of these surveys was expected to allow an in depth study of **current** child mortality from the survival of the initial sample. Such important aspects of child mortality as its age pattern, the effect of body weight at birth and in the first 2 years of life, morbidity, feeding practices, etc. can be explored. **Recent** mortality can also be studied thanks to questions on the survival of the previous births. One interesting feature of the EMIS retrospective questionnaire is that, in addition to the usual questions on the survival of ever born children it includes questions on the survival of the child preceding immediately the current one followed by the survey. This distinction allows a two-level retrospective analysis in addition to the third level of current analysis. The following convention is adopted:

"Current" refers to the child who was the target of the follow-up survey. This child was born at the time of interview.

"Preceding" identifies the child born immediately before the current one.

"Previous" refers to all children ever born, excluding the current one.

"Last" refers to the ultimate child borne by a mother before menopause or secondary sterility, whichever comes first.

It may be asked why bother to try and use the retrospective data when data originating from such a fine

(4 months intervals) follow-up survey is available. The answer lies in the complexity of the analysis of the prospective or current data. The analysis of these surveys is complicated by many problems induced by their unusual methodology.

Based on the same methodology as the IFORD surveys, the EMIS share similar problems. The literature on these surveys directs attention to three major problems: representativeness of sample, omission of births at the construction of the sample, and the progressive depletion of the initial sample.

1.2.1- Representativeness of the sample.

The Senegalese case excepted, all EMIS and IFORD surveys are urban. The following is mainly concerned with the urban surveys. The sample used to study mortality in the city includes only births to resident mothers. Immigrant children (children born out of the city and in-migrated with their parents) under the age of two are excluded. Therefore depending on the importance of immigration and differentials in mortality between immigrants and residents, this approach might give a biased view of mortality in the area under study. In the absence of reliable information on child immigration and related mortality differentials, the EMIS allow the study of the mortality of children born to resident mothers and not the mortality of the whole population below two years of age. In spite of such a reassessment of the objective the problem of representativeness is not completely solved. The IFORD samples consist almost exclusively of children born in the health centers. However, all births to resident mothers do not occur in health centers. Some mothers deliver their children at home while others leave the city for the same purpose. Except in the case of the Bamako study, the EMIS tried to solve this problem by including home deliveries. But as shown in the case of Bobodioulasso (van de Walle, 1984), the effort fell short of taking into account all such cases. However, Houehougbe (1982) contends that "this will not impair the validity of the results or disallow their claim to representativeness at the population level" as long as the births excluded do not represent a very high proportion of the total (p.7). But such a contention would hold only if none of the births at the health centers are omitted and if there are no losses during the follow-up.

1.2.2- Omission of births at the health centers

A significant proportion of health center births is missed by these surveys. In most of these cases, the problem lies in the difficulty of the interviewer finding the mother. Many mothers leave the center before the visit of the interviewer. This constitutes an alarming problem when one knows that most of these mothers left as a result of having lost the baby. The Bobodioulasso survey illustrates this problem further by showing that some mothers, while still at the clinic during the visit of an interviewer, were overlooked. The probability of missing a birth seems to be a function of the difficulty of the delivery. During the first three months of observation, it is estimated that, compared to 5.5% of the "normal" deliveries, 36% of the caesareans are omitted (van de Walle, 1984.). Ouaidou (1984) estimates the coverage of EMIS Bobodioulasso between 75 and 85% of the annual births.

Another problem relating to this one is that some live births are reported as being stillbirths (USED, 1982; Dicko, 1984). In Chapter 2 of this study, we estimate that, in Bobodioulasso, 30% of the reported stillbirths (in the sample of current births) are in fact live births who died.

In the absence of proper correcting procedures, such problems will lead to a serious underestimation of mortality levels. In places where clinic registers are well kept, the information contained in these registers may be useful to correct the current neonatal mortality estimates. This procedure was adopted in Bobodioulasso with relative success (Ouaidou and van de Walle, 1986).

1.2.3- Depletion of the sample.

The mothers in the initial sample are reinterviewed one month after the baseline interview (that takes place in the clinic), then three months later and subsequently every four months. By the time a surviving new born is two years old, seven such interviews would have been conducted if at each visit the interviewer succeeds in locating the mother or guardian. The survey ceases whenever the death of the child is observed. In practice, at each round, a variable number of mothers are lost in the follow-up. In addition to mortality, migration intervenes as a second source of attrition. For example, in Yaounde 12% of the initial sample was lost at the first round and, by the end of the survey, 34% was lost for reasons other than the death of the child. Among the 9747 births recorded in the clinics 3315 were lost while the death of the child was observed only in only 534 cases (Houehougbe, 1981: Table 5, 25). The extent of losses varies from survey to survey according to the configuration of the survey area. In Bamako, for example, 21% were lost at the first round and estimates based on the first four months suggest that as much as 39% of the initial sample will be lost by the end of the survey (Dicko, 1984). In Bobodioulasso the losses at the first round represent 12% (1087 out of 8231 live births) and the total losses 19%. The only documented case where the importance of losses is not critical is the Senegalese rural survey. This led the Final Report of the Bamako seminar to conclude: "the methodology of the EMIS surveys is less problematic in the rural area because of a different social behavior and a lower spatial mobility. In the urban area, the extent of losses is excessive questioning hence the methodology of these surveys" (USED, 1984: 20). One could add that because of the looseness of the spatial distribution - of the population, the rural survey becomes very costly (Mbodj, 1985).

A good question is how all this is relevant to the retrospective analysis. There is some evidence that the previous births to women who were lost to follow-up have experienced a higher mortality (see chapter 3). This is important to keep in mind for the estimation of past mortality. The possibility of using the retrospective reports recorded in the baseline questionnaire to correct the estimates of the follow-up survey was considered at the Bamako seminar (Fargues, 1984). Besides this possible use, interest in the retrospective data is justified by the fact that they can be used as an independent source for the study of child mortality. However, it should be kept in mind that retrospective and prospective data represent two complementary aspects of the EMIS surveys. No serious study of mortality should ignore either aspect.

1.3- Using the retrospective reports as an independent source.

Because the EMIS involve so many problems, a large amount of effort should be devoted to limiting the damage at the stage of analysis. The attempt to use the retrospective data is part of this effort.

During a baseline interview all women are asked questions about the survival of their previous births. The total number of live born children and those now alive, excluding the current one, are counted for each woman. Information on the survival of the immediately preceding pregnancy is also available.

We therefore have two indices of past mortality: the proportion deceased among live births originating from the pregnancy immediately preceding the one that just ended in the current birth, **g**, and the proportion among all previous births who have died, **d**. These are the raw material of retrospective analysis which may deal with differentials or the estimation of levels and trends of child mortality.

1.3.1. Usefulness of the retrospective data for studying differentials

Both indices may be used to study differentials in child mortality across groups of women. Table 1.1 gives an idea of how rich the study of differentials from EMIS data can be. Variations in the two retrospective indices can be compared to variations in the proportion deceased among current births during the 2-year follow-up: **q**. The table contrasts the quite well established impact of mother's education and household income with the multifaceted impact of a behavioral variable: the number of prenatal visits during the last pregnancy. It is apparent that, on the one hand, having prenatal visits during pregnancy increases the chances of survival for the following birth. On the other hand women who lost their preceding child are more likely to adopt prenatal care.

As the different variables are interrelated, reliable conclusions as to the impact of individual variables can be advanced only through multivariate analysis. The survivorship of the preceding child may be introduced as a dummy variable in such an analysis: 0 if alive, 1 otherwise. When one considers all previous births, the method developed by Trussell and Preston (1982) may be used. The method consists in creating an index of child mortality for each woman, index that is used to compare different groups of women or used as the dependent variable in a regression-analysis. One important feature of the index is that it allows the researcher to control for length of exposure of children to the risk of mortality. In the original method women are grouped by marital duration or age (if marital duration is not available) and the index for one women is the ratio of the proportion of her children who have died to the expected proportion deceased for women in the same group. In the EMIS marital duration is not available and as shown in Section 1.4 of this chapter, mother's age is not as good an indication of exposure time as it is in traditional cross-sectional samples. Parity grouping is more adequate because it permits a better estimation of exposure time than age grouping. Parity grouping does not allow as large variations in the estimated exposure time as age grouping does. A better index would therefore be the ratio of one woman's proportion deceased to the expected proportion for women with the same parity. The expected proportion deceased can be calculated using either of the techniques described in chapter 4.

Table 1.1: Proportion deceased among directly preceding births (g), current births (q), and all previous births (d) by various characteristics: EMIS Bobodioulasso.

	Proportion deceased g	q	d
A) Mother's Education			
No Schooling	.1198	.1232	.2306
Primary	.1056	.1118	.1698
Secondary & Above	.0985	.1053	.1394
B) Household Income			
No income	.1469	.5352	.2717
20000 CFA	.1382	.0994	.2316
20 to 50,000 CFA	.1136	.0854	.2162
50000 +	.0791	.0500	.1784
C) Number of Prenatal visits during last pregnancy			
0	.1135	.1500	--
1	.1140	.1581	--
2	.1267	.1235	--
3	.1086	.0924	--
4+	.1391	.0815	--
All Women	.1158	.1251	.2183

1.3.2- Usefulness of the retrospective data for estimating mortality levels and trends.

Attempting to estimate recent levels of child mortality with these data is important for several reasons:

i) the data are available for all EMIS and IFORD surveys. Estimates derived from these sets of data may serve as a good basis of comparison (unique in some cases) for the results of the multi-round survey;

ii) In the cases where it was impossible to carry the follow-up survey to its end, for example in Cotonou, indirect estimation becomes compulsory;

iii) because of the importance of losses and assuming that many losses are related to the death of the child, the indirect estimates based on the initial sample may be closer to the experience of the whole population;

iv) these surveys offer the rare opportunity for evaluating the indirect techniques by confrontation with prospective data.

v) last but not least, the retrospective approach provides the only way of estimating trends from one EMIS survey. And it is unlikely that other EMIS will be conducted in the future given the high cost of these surveys.

However, the retrospective analysis is complicated by the fact that the data made available by the EMIS are not commonly used in demography. Retrospective studies of fertility and mortality are usually based on reports from a random sample of women representing a cross-section of the female population at a given time. The EMIS samples are of a different type. They comprise women giving birth within a definite period and therefore include women who are not only very fertile and still fecund, but also have given proof of their fecundity during the reference period. This type of sample will be referred to as **conditional** (inclusion is conditioned by delivery within the reference period) to distinguish it from the usual random sample which represents the **unconditional** case. The techniques described in chapter 3 and 4 attempt all to deal, each in its own way, with the problems involved in the retrospective analysis but also to take advantage of the particular characteristics of data from conditional samples.

1.4- Characteristics of data from conditional samples.

Data from conditional samples are affected by different types of biases. Some of these are specific to the data collection procedure -- the way sample members are identified and interviewed -- while some others depend on the unusual timing of the interviews and are common to all conditional sample surveys, whatever the data collection procedure. The reference population from which the sample is to be drawn is constituted by all women who give birth in, say, a year. Sample members have to be identified and interviewed at the precise time of delivery. Identification and interview of sample members necessitate special procedures, the adequacy of which will determine the representativeness of the results. The easiest way for identifying the sample appears to be to use the existing civil registration and health systems. That is probably why almost all existing data of this kind were gathered through these systems.

The issue of representativeness raised here is therefore specific to the particular data collection procedure that is adopted. For example, the Solomon Islands' Notification Scheme records the experience of women caught in the nets of the Civil Registration System. As noted by Brass and McCrae (1984), these may differ in many respects from those who should but are not included in the sample. In general, the IFORD and EMIS surveys (or more exactly the urban versions) are limited to women giving birth in the health centers. In most African cities, these are probably not representative of the female population giving birth. EMIS Bobodioulasso attempted to solve this problem by trying to include as many women giving birth at home as possible. The procedure probably improved the coverage relative to other EMIS surveys, but, because of the difficulties inherent to such an approach, a full coverage was impossible (Ouaidou, 1984; Van de Walle, 1984).

Independent of the issue of representativeness are the biases generated by the timing of the interview. Table 1.2 and Figure 1.1 represent the proportions deceased among previous births to reporting mothers in Bobodioulasso. The results are quite surprising. We were expecting that, as is normally the case in unconditional samples, the proportions deceased would increase steadily and predictably with mothers' age because exposure time is a function of the latter. Instead, they are virtually independent of mother's age. In addition to possible errors in age and number of events reported, the flatness of the curve is probably due to thre factors:

i) reduced variation in exposure of children to the risk of mortality. In an unconditional sample women are distributed randomly and continuously over the last birth interval whereas here they are all concentrated at the end of it. Therefore, almost all children have been fully exposed to the high mortality risks of the first year of life. The variation in duration of exposure pertains to a range of ages where mortality is relatively low and changes with age less rapidly than during the first year. In the absence of rapid mortality decline, mother's age will therefore have only a weak effect on the proportions deceased.

ii) relative underexposure of children born to older mothers. The women who stopped childbearing are excluded and, for given age of women, their children are probably on average older than those of older women who continued childbearing. This factor is likely to have a noticeable effect only at ages where the probability of secondary sterility is high; after age 35 for example. As noted by Fargues (1985a): " (i) and (ii) act simultaneously at all ages in opposite directions. The importance of the first decreases while that of the second increases with the respondents' age." (p. 3) However, because (ii) acts mainly at women's ages which correspond to their offsprings' ages where death rates are very low, its impact on the proportions deceased will probably be very weak if indeed perceptible at all.

iii) Selection of young women who lost their preceding birth.

Figure 1.1 shows that the proportions are noticeably higher at the extreme ages: under 20 and over 40. These reflect probably the higher risk of children born to teenage mothers but also age misreporting and errors due to small numbers of women and/or children. Another type of selection bias is also probably acting, at least among the first age group. Young women are included only if they are having a second or higher order birth. This is probably more common among those who lost their preceding birth. The proportion deceased for the first age group is exaggerated by the fact that the preceding child is the only previous birth for most of these women.

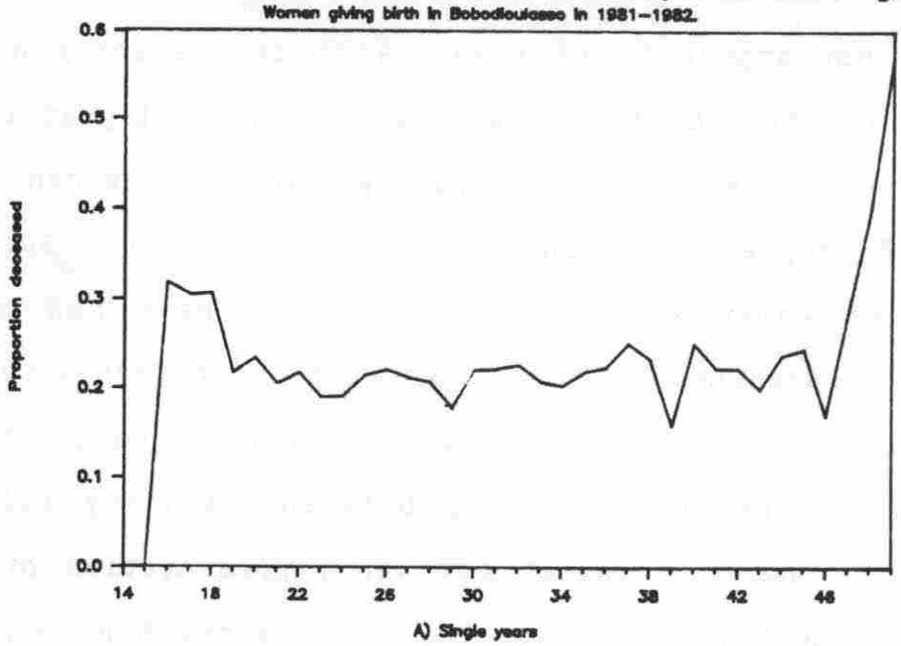
Table 1.2: Proportions deceased by mother's age group: Women giving birth between April 1, 1981 and March 31, 1982 in Bobodioulasso.

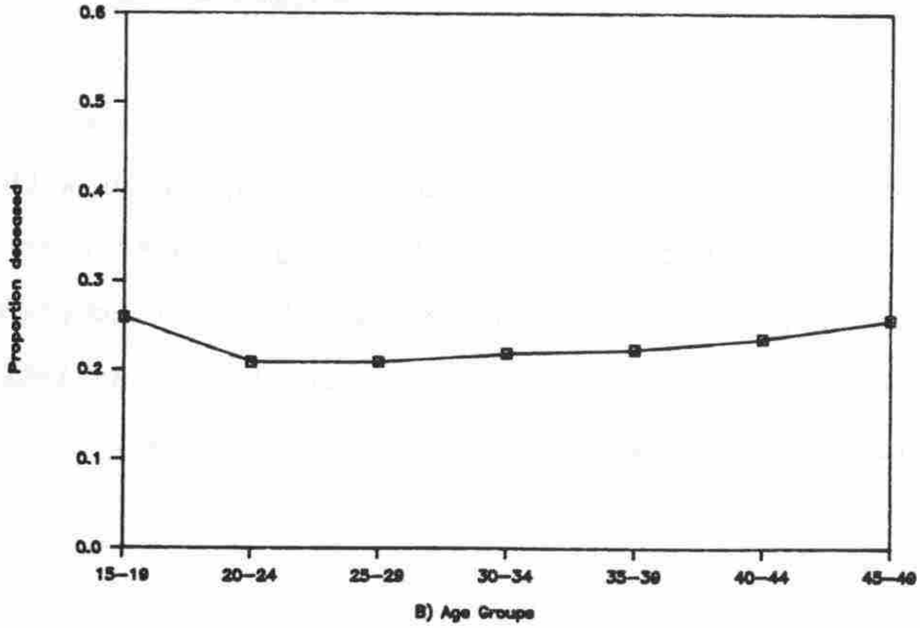
Age group x	Number of women W_x	Number of ever born B_x	Proportion deceased d_x
15-19	1879	557	0.2585
20-24	2418	3779	0.2085
25-29	2090	7272	0.2094
30-34	1010	5520	0.2190
35-39	593	4290	0.2231
40-44	171	1478	0.2355
45-49	22	183	0.2568
ALL AGES	8183	23079	0.2178

Note: Age less than 15 and missing values are excluded.

FIG 1.1: Proportion of dec. children by mother's age:

Women giving birth in Bobodioulasso in 1981–1982.





EMIS data are affected by different types of biases and it does not appear to be possible to tell exactly how the result of different biases will affect mortality estimates. First of all these surveys miss high mortality women at the stage of sample selection (see Section 1.2.2.). Secondly, they lose women whose previous births have had higher mortality (see Chapter 3). In general, high fertility women are over represented in conditional samples. Partly because of the positive relationship between fertility and child mortality, one can suspect an over representation of women who lost their preceding child mainly among high mortality groups. For example, the extremely high proportion deceased for age group 15-19 (in Table 1.2) suggests a special bias, at least for this age group.

The EMIS Bobodioulasso data alone do not allow us to settle the issue of selection bias. Comparison with other data and also, probably, simulation may help sort the issue out, however, such a simulation will be very complex. It will have to make mortality and the length of the interbirth interval vary with age and parity of the mother.

CHAPTER 2

RELIABILITY OF REPORTS ON CHILDREN EVER BORN, CHILDREN SURVIVING AND CHILDREN DECEASED

2.1 - Introduction

Because of the relative deficiency of vital registration systems in many developing countries, most of the knowledge we have on infant and child mortality in the developing world is achieved through demographic surveys. Demographic surveys may be "multi-round" or "single round". In single round surveys the study of mortality is made possible by retrospective questions on events that took place before the interview. Retrospective surveys may be classified into two groups according to the type of questions asked. The **classical** approach consists in asking questions on the cumulative number of events while the more recently developed **pregnancy history** approach probes for the outcome of each single pregnancy. The WFS programme used the second approach on an unprecedented scale and in a variety of national and socioeconomic settings. From the results of this large scale operation, it appears that, as far as the coverage of vital events is concerned, detailed pregnancy histories constitute an improvement relative to the classical approach (Hobcraft, 1984; Preston, 1985).

Probing for the outcome of each pregnancy makes a difference because it helps record events that might otherwise be omitted. However, despite the laborious efforts deployed to record detailed pregnancy histories, there are still persistent signs of omissions. In general, because of such omissions, retrospective surveys tend to yield less reliable data relative to other demographic investigations (Committee on Population and Demography, 1981).

In multi-round surveys the same population is interviewed repeatedly. The events that may have occurred between rounds are ascertained by asking retrospective questions to persons interviewed at previous rounds. Omissions do occur in these surveys despite the fact that the recall period is limited to a few months. And the damage may be highly significant: In Sine Saloum (Senegal), Cantrelle (1969) estimates that as a consequence of these omissions the infant mortality rate could be underestimated by as much as 37% without proper correcting procedures.

Multi-round surveys show that the under reporting of deaths is highly selective. Children who died at a more advanced age are less likely to be omitted than those who died earlier in life. In other words, post-neonatal deaths seem to be better reported than neonatal deaths (Cantrelle,1969; Garenne,1984). In Garenne's study, 17% of post-neonatal deaths and 81% of neonatal deaths were omitted when women are asked questions on the events that occurred since the last round (which took place less than 12 months previously). It is highly doubtful that with such a short period of retrospection mothers who omitted some child deaths did so owing to forgetting.

Recent methodological developments in Sine-Saloum (Senegal) shed more light on this issue. Garenne 1984 shows that, in the Sine-Saloum multi-round survey, the frequency of visits is not a determinant factor in the omissions of vital events. He concludes:

> "these results suggest that the major cause of omission of early child deaths in multi-round surveys is neither a reticence to talk about deaths, nor the fact that the child is not considered as a member of the family until he is baptized nor the fact that early deaths are confused with stillbirths: the survey technique is the major factor and, in fact, the type of question determines the quality of the response." (p 13).[our translation]

Other studies have found the adequacy of the questions asked to be a major factor in determining the quality of responses (Quandt, 1973; Gibril 1974; Thompson et al., 1982). However, by contrasting survey technique and the confusion between early child deaths and stillbirths, Garenne overlooks the fact that the latter may be a simple result of the former. This **confusion** may well originate from inadequate survey technique (for example flaws in the formulation of questions) or from a genuine absence of discrimination (voluntary or not) by mothers between the two types of events. Whatever its source, this **confusion** seems to stand as a major cause of omission of early child deaths. Evidence of this fact was noted while the EMIS fieldwork was still ongoing, both in Bamako and Bobodioulasso (Dicko, 1984; USED., 1982).

The EMIS type surveys differ from classical multi-round surveys by their approach. Here a birth cohort is followed for two years and the changes are recorded concomitantly. As mothers are interviewed at the time of birth of the child who is the object of the follow-up survey, the EMIS prospective data are free from errors due to "memory failure" and therefore permit the study of what happens in the absence of pure recall errors. On the other hand, because Brass type questions were asked at the initial interview, the EMIS also allow a retrospective approach to the study of mortality. The sample of mothers gave similar reports to the clinic nurses at the time of delivery. The maternity registers therefore constitute an additional source of information on the cumulative number of events that can be compared to the survey reports.

This chapter investigates the reliability of responses to Brass-type questions by comparing these two sets of independent reports given by the same women. It addresses the differentials and determinants of reliability and attempts to give further evidence on the importance of the confusion of early deaths with stillbirths both in the retrospective and prospective surveys.

2.2 - **The sources**

The study of reliability requires at least two trials of the same measurement procedure. Women's reports to the survey interviewer on the one hand and on the other, those recorded by the nurse (who attended the delivery) in the maternity register yield two different sources for the measurement of the number of children ever born, children surviving and children dead. This section describes the information recorded by the two sources (**survey** and maternity **registers**) and the matching procedure.

The women interviewed by EMIS Bobodioulasso were asked the following questions:

- how many live births have you ever had?
- how many of them are still alive?
- have you ever had any stillbirths? If yes, how many?
- have you had any abortions? If yes, how many?

These questions relate to previous events. The pregnancy leading to the current birth (the target of the follow-up survey) was in principle not included. The first interview where the baseline questionnaire is completed took place as soon as the interviewer learned of the event, and in most cases within the three days following the delivery while the mother was still in the maternity ward. The baseline questionnaire contains all the retrospective information collected in the sample.

Similar information was recorded in the maternity registers by the nurses for all women who delivered in the two major maternity clinics (the Hospital and Guimbi), or went there for help just after having given birth at home. The following data were recorded in the maternity registers:

- total number of pregnancies including the current one
- number of previous live births still alive,
- number of children who died and,
- the number of intrauterine deaths is as the difference between the number of pregnancies minus 1 and the number of live births. It is not recorded but calculated.

By comparing and contrasting these two sources one can study the discrepancies between reports. The maternity registers were systematically recopied. Because of lack of time it was impossible to cover all of the twelve survey months. The registers of January, February and March 1982 were therefore not consulted. The records for the nine months covered were matched later using the date of delivery, the name of the mother and the sequential number of the maternity registers (which was usually copied on the survey questionnaire). All ambiguous cases are excluded. In total 2844 women were matched. The maternity records were then merged with the baseline questionnaire in one file including background information reported at the survey such as age, education, ethnic group, etc.. The following variables can be defined in both sources:

SURVEY	REGISTER	DESCRIPTION
Ps	Pr	Number of pregnancies
Bs	Br	Children ever born
Ss	Sr	" surviving
Ds	Dr	" deceased
Us	Ur	Intrauterine deaths

The quality of the two sources may differ for a variety of reasons. One possible reason is a difference in the nature of the questions asked by the nurses and the survey interviewers.

But we have no a priori reasons to believe that, as a consequence of this difference, one source should be better than the other. It is true that the survey interviewers were trained to do the job according to a demographer's perspective while the nurses were not. But on the other hand, it may be true that respondents were more motivated in making the effort necessary to give a correct answer to the nurse. A woman who enters the maternity and is awaiting an event as important as the birth of a child may be more cooperative if she expects her answers to be of any importance for the help she needs from the nurse. In other words, she may feel concerned by the nurse's questions and therefore be more cooperative than she would be with the survey interviewer who is a stranger in the maternity and who sometimes inspires suspicion.

Because data entry and data collection were done in parallel it was possible to correct major deficiencies in the survey reports. The data entry program contains different consistency checks. Some of these are related to the number of vital events. For example the number of deliveries is compared with the sum of the number of live births and the number of stillbirths. When the outcome of the preceding pregnancy is a stillbirth or a miscarriage, it is verified whether the reported cumulative events contain at least one stillbirth or one miscarriage. Whenever such tests were positive an error message was issued that would describe the type of inconsistency and the interviewer was sent back to settle the issue with the respondent. The nurses recorded the number of events with no probing whatsoever. For this reason and assuming that a little probing is better than no probing at all, one may expect the survey reports to be more complete.

2.3- <u>Measuring reliability</u>

Definition.

Reliability concerns the extent to which repeated measurements of the same phenomenon give the same results. In the realm of social investigation the measurement of any phenomenon always contains a certain amount of error. Despite this unavoidable presence of error, repeated measurements tend to be consistent with each other. "This tendency toward **consistency** found in repeated measurements of the same phenomenon is referred to as **reliability.** The more consistent the results given by repeated measurements, the higher the reliability of the measuring procedure; conversely, the less consistent the results, the lower the reliability." (Carmines and Zeller, 1979; pp 11-12). This definition implies that unreliability is always present to a certain extent and that the measurement of reliability itself is not an exception because consistency of repeated measurements may simply be due to the repetition of the same error.

Application to EMIS Bobodioulasso.

When the information recorded by the survey and the information recorded in the maternity registers differ we can be sure that at least one error has been committed. An error may have been committed by the respondent, the interviewer, the nurse or may have been introduced at the much later stage of data entry and data processing. All the different steps that lead from the response of the interviewee to the final value in the researcher's data set may be involved. What does seem certain is that inconsistency of the sources cannot be explained by "memory failures" due to time lag between the inquiries: both took place almost simultaneously; however, both could be equally affected by the same errors.

A first attempt has been made to evaluate the survey data by comparing them with the maternity records (USED, 1982). That study is limited to 474 women who gave birth in April and May, 1981. Despite its preliminary character it leads to two important conclusions:

a) it appears for example that inconsistency of reports increases with parity;

b) the confusion between early infant mortality and intrauterine mortality also seems obvious.

There is, for example, inconsistency affecting at least one variable in 81% of the cases where one of the sources has recorded a miscarriage or a stillbirth. One problem with the preliminary study is that it is based on the simplifying assumption that discrepancies between sources stem from the omission or misclassification of one event by either source (page 5). A typology of errors is devised which is based on the assumption that one source has recorded the true value. For example errors of type D are labeled "omission of an abortion or a stillbirth by the hospital". We want to argue that in this particular case one cannot be sure whether we face an omission by the hospital or an exaggeration by the survey; the events reported here are not individually identifiable.

All our variables are subject to errors. One may hypothesize that one variable should be less affected by errors than another but not that either source has recorded the true value. Therefore this reliability study is limited to evaluating the consistency of reports. It is impossible to tell precisely the source of particular types of errors. But, by identifying the characteristics of women who are the most likely to commit errors it will be possible to make reasonable inferences about the sources of error.

Indices of reliability.

Table 2.1 shows that, as one could have expected, the survey recorded on the average more events than the maternity registers. The only exception is the number of children dead - the registers recorded 50 events more than the survey. The means of our variables are not significantly different except for the number of Intrauterine deaths: U. This result is consistent with the existing evidence that reports of non-live births are less reliable than those of live births (Becker and Mahmud, 1984). It also seems that, in both sources, the number of intrauterine deaths is severely under reported. The proportion of intrauterine deaths among all pregnancies is .071 in the survey and only .055 in the registers.

TABLE 2.1: Means of selected variables by source.

SOURCE	Pregnancies P	Ever born B	Surviving S	Dead D	Intrauterine deaths U
SURVEY					
Number of events	8163	7763	6103	1649	586
Mean/woman	2.95	2.74	2.16	0.58	0.21
REGISTER					
Number of events	8056	7726	6030	1696	446
Mean/woman	2.89	2.73	2.13	0.60	0.16
R - S	-0.06	-0.01	-0.03	0.02	-0.05*

* significant at the 5% level, two-tail test.

TABLE 2.2: Distribution of mothers by the deviation of their registration report from that of the survey.

R - S	B	S	D	B	S	D
	FREQUENCIES			PERCENTAGES		
-(2+)	38	28	24	1.3	1.0	0.9
-1	135	103	109	4.8	3.7	3.9
0	2489	2600	2516	88.1	92.3	89.3
+1	128	72	135	4.5	2.6	4.9
+(2+)	35	15	30	1.2	0.5	1.1
TOTAL	2825	2818	2816	100.0	100.0	100.0
C*	-	-	-	78.3	80.3	82.0

* percentage with an absolute error of 1 among mothers with inconsistent reports.

It is well known that intrauterine deaths are poorly reported - they are often omitted in pregnancy histories and even the pregnancy itself, when it ends early, may remain unperceived. In his review of studies of the occurrence of fetal deaths, Leridon concludes:

> "About 25% of the pregnancies in progress after four weeks of gestation (since the last menstrual period) end in an intrauterine death. However this estimate can be obtained only by means of a life table of intrauterine mortality, since rates obtained by purely retrospective data are usually on the order of 12 to 15%." (Leridon, 1977 p76)

If, as argued by Bongaarts (1982), the occurrence of intrauterine mortality is stable across populations then reports of intrauterine deaths are quite deficient in both sources. The total number of pregnancies (P) is obtained by adding the intrauterine deaths, U to the number of live births, B. Because U represents a small portion of P, the reliability of the latter is only slightly affected by that of the former.

One question is why death reports are lower in the survey. As the events are not identifiable individually any answer to this question may only be tentative. It is common sense to expect that the survey should have recorded more deaths as is the case for all other events. The paradoxically higher number of deaths recorded in the maternity registers may be a consequence of misclassification errors. Misclassification may work in both senses: nurses may have wrongly recorded some intrauterine deaths as live births who died; on the other hand, some child deaths may have been recorded as stillbirths by the survey interviewers. It is also very likely that, by asking questions on stillbirths and miscarriages, the survey prevented some of these events from being reported erroneously as live births who died. However, misclassification errors do not seem to explain all the difference. The fact that the difference in the number of intrauterine deaths exceeds the difference in the number of postnatal deaths - it is about 3 times as high- suggests differential levels of omission as an additional factor. From Table 2.2 we see that most women reported their cumulative number of events consistently. The percentages of women reporting exactly the same number of events in both sources is 88 for B, 92 for S and 89 for D. It also appears that most discrepant cases involve a difference of only 1 event: 78%, 80% and 82% of women who reported differently the number of events present a difference of one unit in, respectively, B, S and D.

All the information we need to measure the reliability of a given variable is contained in the cross-tabulation of the different values of that variable in each source. The indices we are going to use are simple summaries of that information. For a given variable, let P_{ij} be the proportion classified in category i by the survey and in category j by the maternity registers. The main diagonal contains all cases with exact agreement, or $\sum P_{ii}$ over i=1 to n, where n is the number of categories. The proportion classified in i by the survey is $P_{i.} = \sum P_{ij}$ over j=1 to n. Similarly, for the registers we have $P_{.j} = \sum P_{ij}$ over i=1, ..., n.

Our first index of reliability is the **percentage giving inconsistent reports**:

$$I = (1-\sum P_{ii})*100$$

The usefulness of I as an index of reliability is due both to its value as a descriptive term and to the simplicity of its interpretation. But it also has a major drawback - it ignores randomness. Consistency may occur by mere chance even in the presence of totally unreliable measurement.

One way to overcome this problem is to use Kappa:

$$K = 1 - \frac{\text{observed disagreement}}{\text{expected disagreement}}$$

(Macdonald et al., 1978; Omuircheartaigh and Marckwardt, 1980; Omuircheartaigh, 1984). The expected disagreement is the complement to 1 of the product of the sum of the two marginal distributions. Expected agreement = $\sum P_{i.}*\sum P_{.i}$ and expected disagreement = 1-expected agreement.

For the sake of ease of interpretation we use a closely related index: **R** = (1-K)*100.

The following formula is used to compute this index:

$$R = \frac{1 - \sum P_{ii}}{1 - \sum P_{i.}*\sum P_{.i}}$$

Thus **R** is an approximation of the **proportion responding randomly** (Ryder and Westoff, 1971). R is preferable to I because it does not credit random agreement as consistency (idem. p.361). But both indices have one major limitation; they fail to take into account the magnitude of error.

The following discussion is limited to the 3 variables relating to live born children only; B, S, and D. The number of intrauterine deaths, U, is small and a finer level of analysis is not much instructive. P is also dropped. The ultimate aim being to study the effect of errors on the proportion deceased, the raw material of indirect estimation.

The effect of age and parity.

It is common sense to expect that the larger the number of events to be recalled, the higher the probability of erring when reporting them. In addition, the more distantly an event has occurred in the past, the higher the probability of misreporting it. As far as the reporting of previous live births is concerned, evidence of both types of problems does exist. Using the Indian National Sample Survey data, Som (1959) finds recall lapse to be a function of the time elapsed between the date of the event and the date of interview. Comparing the retrospective reports in the American Current Population Survey of 1965 with the vital statistics, Sirken and Sabagh (1973) show that the completeness of survey estimates decreases with increasing length of the retrospective period. In other words, vital events are more likely to be omitted the longer the period of retrospection. More recently, Lacombe (1970) in Senegal and Becker and Mahmud (1984) in Bangladesh found that recent events are better reported than earlier ones. Misreporting of live births was also found to increase with parity (Omuircheartaigh and Marckwardt, 1980; Omuircheartaigh, 1984; Becker and Mahmud, 1984).

The high correlation between age and parity makes it difficult to disentangle their effect on misreporting. Older women are not only higher parity women but also, on the average, their births have occurred farther back in time.

The results in Table 2.3 confirm these earlier findings: I increases with both parity and age. S appears to be the most reliable according to both indices. D seems more reliable than B if I is used as index, but this is simply a reflection of the deficiency of I as a unique measure of reliability.(It is highly affected by skewness in the variables.) Here category 0 contains 65% of the sample for D and only 25% and 28% for B and S respectively. Thus we see that the higher the level of skewness the lower the value of the index. Therefore I should not be used to compare variables with highly different levels of skewness. R, a better index of reliability, shows D to be the most affected by errors.

Decomposition of the errors affecting the number of live births B.

B is the sum of S and D. Therefore errors affecting B may have only two sources:

- uncompensated errors in S,
- uncompensated errors in D.

This decomposition is only an approximation for at least one reason: except for S, the variables are not derived the same way in both sources. From the survey data, B is derived from an independent question and D is computed as the difference between B and S. However, in the maternities direct questions were asked only for S and D; the two are added afterwards to give B. The estimates are presented in Table 2.4.

Table 2.3: Proportions giving inconsistent reports (I) by age and number of events reported in the survey

	Ever born Freq	I	Surviving Freq	I	Deceased Freq	I
NUMBER OF EVENTS						
0	703	5.4	804	3.9	1829	6.0
1	488	6.8	558	8.1	580	12.8
2	379	8.4	434	5.5	239	25.5
3	312	14.4	343	11.1	114	31.6
4-5	469	17.3	422	10.7	54	35.2*
6-8	380	21.8	232	13.4	-	-
9 +	94	25.5	25	16.0	-	-
AGE GROUP						
< 20	656	3.7	656	3.2	656	2.3
20-24	850	9.1	850	7.4	850	7.8
25-29	733	15.3	733	8.9	733	14.1
30 +	586	21.0	586	11.8	586	19.8
ALL WOMEN						
I	2844	11.9	2844	7.7	2844	10.7
R		13.8		9.3		20.0

* 4 and above.

NOTE: Because of missing values the total number of women is greater than the sum of frequencies.

Table 2.4: Decomposition of errors in reporting the number of children ever born.

		Number of women for whom: B is consistent	B is inconsistent
S consistent	D is consistent	2407	0
	D is inconsistent	0	193
S inconsistent	D is consistent	0	109
	D is inconsistent	76	33

Number of cases where B is in error:	335
Percentage due to errors on S:	32.5
Percentage due to errors on D:	57.6
Percentage due to non-compensating errors on both S and D:	9.9

A variable is consistent if both sources report the same value for that variable. From this Table it appears that 58% of the errors in B are due to errors in D; 32% are due to errors in S and, 10% to errors in both S and D. It is also interesting to note that in about 70% of the cases where both S or D are in error (109), errors in the two variables compensate each other to yield an error free B.

To summarize the findings in this section, we can say that the vast majority of mothers report consistently the number of their offspring. Non-live births are more likely to be misreported than live births and, as a consequence, U is the least reliable variable. Live births who died are also more poorly reported than the surviving ones. Therefore unreliability of reports on live births is more a function of reports on D than reports on S.

Defining the symbol " $<$ " to mean "is less reliable than", we can classify the variables derived from the retrospective data in the following way:

$$U < D < P < B < S$$

according to **R** which is equal respectively to 30.9, 20.0, 15.0 13.8 and 9.3.

2.4- <u>Differentials in reliability: the outstanding role of misclassification.</u>

An important aspect in the study of reliability is to see how different subgroups of respondents perform relative to each other. The study of differentials is also important because, in the case of retrospective reports on cumulative numbers of events, it is perhaps the only way to glean insights about the sources of misreporting. To our knowledge reliability studies of demographic data thus far have been limited to univariate analysis. In this study a mulivariate approach has been taken.

With a simple univariate approach, it is difficult to tell whether the different factors have any direct impact on reliability. The effect noted for any single variable may be a spurious one. To properly infer the effect of a given factor one should control for other factors; multivariate analysis is required. This type of analysis is also integral to this study because, unlike our two summary indices, it helps take into account the magnitude of error.

The model.

As we are interested in a simple estimation of the relative impact of selected variables, ordinary least squares regression is used. To take into account the magnitude of error, the absolute value of the difference between sources for a particular woman is used as the dependent variable. The variable AGE is excluded from the model to avoid the problem of multicollinearity; AGE is highly correlated with the number of events. The effect will be a slight upward bias in the coefficients for EVENTS, which picks up some of the effect of AGE. It should be noted that the coefficients of the variables which are not involved in this multicollinearity are not affected. Table 2.5 give the list and description of the variables.

Table 2.5: List of variables used in the analysis and their description.

Variable	Description
Clinic	The 2 major maternity clinics are Guimbi and the Hospital.
Interviewer	Numbers 1, 2, 7, and 15 are distinguished from the others who represent the omitted category in the regression. These four interviewers conducted the majority of interviews in both maternity clinics.
Ethnic Group	The 2 major groups are Bobo and Mossi. Others is the omitted category. Bobo and Mossi are entered as dummy variables in the regression.
Origin	Is the place of birth of the woman: if born in Bobo, 0 otherwise.
State of mind	If the woman has lost her new born, her state of mind is "Unfavorable", MINDST = 1 otherwise, MINDST=0.
Schooling	In the regression, SCHOOLING represents the number of years of schooling completed.
Events	According to the regression considered is the number of ever born, the number surviving or the number dead.
IUM	Dummy variable = 1 if at least one intra-uterine death is reported, 0 otherwise.

The results in Table 2.7 show that the model does a better job of explaining the variations in the quality of D (R^2 = .171) than in the quality of S (R^2 = .027). The lower explanatory power for S may be due to the fact that S is a highly reliable variable and most of the variation in its reliability is probably due to random fluctuations and other factors not taken into account by the model e.g. data entry or copying errors. Tables 2.6 and 2.7 respectively present the univariate and multivariate results. Only 3 variables have a significant impact on the reliability of reports: IUM, INTERVIEWER and EVENTS.

The role of misclassification errors.

In his evaluation of the parity data collected on birth certificates in Bombay, El Badry (1962) finds that "mortality, both prenatal and postnatal, features as the paramount reason for deficiency in the registration reports" (pp 341-342). In our data these two factors are also very important. Mothers who reported no mortality in the survey have, on average, given more consistent reports. Mothers who reported only postnatal mortality have also done better than those who reported at least one intrauterine death (see univariate results Table 2.6).

For many mothers (about 17% of the sample) the pregnancy preceding the current one is the only other one they report having. For that reason **I** and **R** are given by the outcome of the preceding pregnancy. The two indices behave the same way when mothers are classified according to the outcome of the preceding pregnancy - those who reported an intrauterine or a postnatal death gave less reliable reports. The fact that the reliability of S is not sensitive to mortality reports gives support to the assumption that intrauterine deaths or postnatal deaths are not reported as (confused with) surviving children. Misreporting of intrauterine deaths can affect the reliability of B only through their confusion with live births who died. Existing evidence suggests that this confusion may go both ways: some fetal deaths may be reported as live births and vice versa (Gibril, op. cit.; Macdonald et al., op. cit.)

To estimate the effect of the **confusion of intrauterine and early child mortality**, the dummy variable IUM is introduced in the regression. This variable has no significant effect on the reliability of S. But the coefficients for both D and B are statistically significant far beyond conventional levels. IUM coefficients for D are at least twice as high as any other coefficients in the regression. It appears that many errors in surveys originate from discrepancies between the survey concepts and those used by the populations in their everyday life. Both interviewer and respondent may misunderstand them and the consequences may be disastrous (Quandt, 1973; Gibril, 1974).

Even if the difference between intrauterine mortality and infant mortality is conceptually clear, distinguishing them may be very difficult in real life. For the average mother, there is not a big difference between a stillbirth and an early child death. Both lead to the same outcome: the loss of the product of nine months of anticipation. Births followed by early deaths may be omitted in retrospective surveys not because they are forgotten but merely because the respondent does not see the need for reporting them except perhaps as a stillbirth. The fact that even multi-round surveys do well, as far as measuring neonatal mortality is concerned, only when they include probing into the outcome of previously recorded pregnancies is revealing in this context. (For the Senegalese experience see Garenne, 1982) In surveys where the recall period is limited to a few months, it is very unlikely that mothers **forget** births followed by death between rounds. As we shall show in the next section, even if the retrospective period is limited to only a few hours after the birth of a child, the event is still very likely to be recorded as a stillbirth if that child is dead before the interview.

It is also clear that a wrong question will yield a wrong response. The interviewers sometimes misinterpret the survey definitions and end up asking inadequate questions.

interviewer effect.

The interviewer codes suggest that at least 18 interviewers were involved in the survey, but that only a few of them have conducted most of the interviews. One interviewer (number **one**) did 85% of the interviews in Guimbi and three (numbers **two, seven** and **fifteen**) did 91% of the interviews in the Hospital. Because Guimbi reports are excluded from the regression, interviewer number one does not appear in the regression. The omitted category includes all other interviewers. The reliability of S is not noticeably affected by interviewer differences. These differences are only significant for D and subsequently for B. This confirms the fact that the quality of work differs from interviewer to interviewer and that all errors should not be attributed to respondents.

Differences between interviewers also appear to be important when one compares the quality of data from the 2 maternity clinics. In Table 2.6 the variable CLINIC is a proxy for the relative differences in survey and registration procedures between the two maternity clinics. The surprisingly higher reliability in Guimbi (indices about twice as low) seems to be an indication of the possibility (noted earlier by USED, 1982) that interviewers in that clinic may sometimes have copied the maternity registers. One other indication is that, in Guimbi, the mean number of events is very similar in both sources, particularly for the number of intrauterine deaths, which is the least well reported. In Guimbi the mean number of intrauterine deaths does not vary significantly from one source to the other. The variable IUD is not significantly different: 0.14 from the survey and 0.13 from the registers. By contrast, in the Hospital, the two means are respectively 0.24 and 0.17 and the difference between the two is highly significant. Therefore we conclude that two sources are not independent for mothers who gave birth in Guimbi. For this reason these women are excluded from the multivariate analysis.

Effect of the Number of events to be reported.

The univariate results for the number of events are presented in Table 2.3 which shows that the number of events has a negative effect on reliability. In the regression, the coefficients for EVENTS are positive and are all significant at the 1% level. The differential effect of this variable is also remarkable. An additional event increases the absolute difference between the sources by about 0.03 for S and B. The effect on D is almost 4 times as high. This gives evidence that children who died are more subject to misreporting errors as the number of events increases. However, because the "events" considered are different in each regression, the greater effect of that variable of D could also be explained by the fact that fewer deaths are reported then births and survivors.

Schooling.

Previous reliability studies find that mothers with some formal education report their parity slightly better than those with no formal education (El BADRY, 1962; Omuircheartaigh, 1984; Macdonald et al., 1978; Becker and Mahmud, 1984). The same relationship between schooling and reliability is found here. The two indices are higher for mothers with no formal education for both S and B. D is less sensitive to the level of education. In the regression, the variable SCHOOLING represents the number of years of schooling completed. In the presence of the other factors schooling has a negative but very weak effect; the greater the number of years completed the higher the quality of reports but none of the coefficients is significant at the 5% level. Three reasons may explain the negative relationship between schooling reliability:

- mothers with higher levels of education are more likely to have birth certificates for their children, which may be very helpful in preventing recall errors;
- they may have lower fertility and fewer events to report and therefore be less likely to make errors in reporting those events;
- the concepts used in demographic surveys may be more meaningful to mothers who spent more years in school.

The socioeconomic variables in the regression have but a weak impact on data quality. The major source of errors seems therefore to be technical and has its foundation in the interaction between interviewers and respondents.

TABLE 2.6: Subgroup differentials in inconsistency.

		Freq	S I	S R	D I	D R	B I	B R
CLINIC	HOSPITAL	1914	8.9	10.7	12.9	24.2	14.1	16.4
	GUIMBI	907	5.1	6.2	5.7	10.9	7.0	8.1
INTERVIEWER	ONE	771	4.3	5.2	5.1	9.7	6.0	6.9
	TWO	1098	8.7	10.4	11.8	21.6	12.5	14.4
	SEVEN	234	14.5	17.2	24.4	43.6	24.4	27.9
	FIFTEEN	394	6.4	7.9	7.4	14.5	10.2	12.1
	OTHER	319	9.5	11.7	13.9	26.3	16.9	19.9
ETHNIC GROUP	BOBO	455	5.7	6.7	10.3	19.2	11.4	12.8
	MOSSI	665	8.5	10.2	8.5	16.7	10.9	12.5
	OTHERS	1677	7.8	9.5	11.7	21.6	12.9	14.6
ORIGIN	BOBODIOUL	718	5.7	7.3	8.4	18.6	10.0	12.5
	ELSEWHERE	1883	8.5	10.8	11.8	20.9	12.5	14.3
STATE OF MIND	FAVORABLE	2634	7.7	9.8	10.8	20.2	11.7	13.5
	UNFAVORA.	190	8.9	12.0	9.5	18.7	14.2	17.9
SCHOOLING	WITHOUT	2126	8.4	10.0	11.5	20.2	13.0	14.7
	PRIMARY	486	4.7	5.9	8.9	21.3	8.6	10.4
	SECONDARY	158	4.5	6.6	4.5	15.7	7.6	10.6
REPORTED IN SURVEY	NO MORTAL.	1618	5.9	7.7	3.4	-	6.0	7.9
	POST. ONLY	737	9.5	11.0	14.3	25.0	13.7	15.5
	PRE. ONLY	203	12.8	14.9	26.6	-	27.1	31.4
	POST + PRE	266	10.4	11.8	33.5	46.9	31.2	34.7
OUTCOME OF PRECEDING PREGNANCY	ALIVE	1769	8.9	10.6	11.8	20.5	13.0	15.0
	DEAD	229	10.8	13.5	18.0	29.4	17.5	21.0
	IUD	147	10.9	14.3	25.9	40.1	27.2	32.1
ALL WOMEN		2844*	7.7	9.3	10.7	20.0	11.9	13.8

* Because of missing values Total is greater than sum of frequencies.

TABLE 2.7: Results of regression analysis.

INDEPENDENT VARIABLES	DEP. VARS=Abs. Diff. Bet Sources for: SURVIVING	DECEASED	EVER BORN	MEAN
BOBO	-0.0801*	-0.0459	-0.0560	0.130
	(-2.081)	(-1.355)	(-1.210)	
MOSSI	0.0080	-0.0333	-0.0239	0.228
	(0.270)	(-1.272)	(-0.672)	
ORIGIN	0.0056	0.0311	0.0108	0.732
	(0.187)	(1.187)	(0.301)	
MINDST	0.0149	-0.0495	0.0421	0.034
	(0.224)	(-0.849)	(0.530)	
SCHOOLING	-0.0081	-0.0030	-0.0060	1.284
	(-1.695)	(-0.722)	(-1.049)	
INTERVIEWER				
TWO	-0.0117	-0.0982**	-0.1431**	0.586
	(-0.288)	(-2.739)	(-2.923)	
SEVEN	0.0405	0.0475	0.0246	0.130
	(0.805)	(1.073)	(0.406)	
FIFTEEN	-0.0359	-0.1379**	-0.1589**	0.183
	(-0.767)	(-3.342)	(-2.822)	
EVENTS	0.0275**	0.1069**	0.0318**	-
	(4.577)	(9.632)	(5.321)	
IUM	0.0564	0.3105**	0.3316**	0.204
	(1.631)	(11.532)	(8.830)	
R^2	0.0274	0.1713	0.0919	
F	4.835	35.456	17.359	
NUMBER OF CASES	1726	1726	1726	

* significant at 5% level.
** significant at 1% level.
NOTE: t statistics between parentheses.

2.5 - Misclassification errors in the follow-up survey.

At the baseline interview mothers were asked the outcome of their delivery. As the retrospective period extends to only a few days or hours, one can assume that errors due to recall lapse do not affect the quality of answers to that question. In other words, mothers have not had the time to forget the pregnancy outcome. One interpretation of the philosophy behind the advocacy of multi-round surveys is expressed in the following by Arretx and Somoza (1973):

> "It may be presumed that if a population were surveyed permanently, say daily, omission of births and deaths would be almost impossible. These events would be known on the very same day or on the day following their occurrence and would not be omitted owing to forgetting by respondents. The problem would, in all likelihood, be solved." (p6)

EMIS Bobodioulasso follow-up data contradicts this belief (see Tables 2.8 and 2.9). Even if the interview is held a short time after delivery, the time lag is always enough for certain live births who died to be omitted. If a child dies before the interview, however close the interview is to the time of birth, the event is very likely to be reported as a stillbirth. This unexpected source of omission of live births is far from negligible because of the high mortality of the first days of life.

In the following discussion, it is assumed that the outcome recorded by the nurse who attended the delivery is the correct one. By comparison with the registers, the survey reports were corrected to take into account misclassification of deaths as stillbirths (See van de Walle and Ouaidou, 1986). This was done for the nine months March through December, 1981; the 1982 registers were not consulted. When a woman reporting a stillbirth to the survey is identified in the Hospital register, her two reports are compared. If the nurse who attended the delivery has recorded an outcome that is different from the one recorded by the survey, the survey report is corrected to match the outcome recorded by the attending nurse. In some cases the correction was done directly on the computer files and there is no trace of it. But in about half the cases the mothers for whom such a correction was done are identifiable; this information is presented in Table 2.8.

Thirty percent of the reported stillbirths for these women are erroneous. Assuming that the same pattern of misreporting prevails among the half of corrections who are not identified on the Hospital registers, we attempt to estimate in Table 2.9 the effect of the misclassification errors on the mortality rates of the follow-up survey.

TABLE 2.8: Misreporting of early child deaths as stillbirths in the follow-up survey: EMIS Bobodioulasso.

Month of Delivery	# of women reporting a stillbirth to survey who are identified in Hospital registers	Number of Reported	Stillbirths Erroneous
APRIL	8	8	3
MAY	12	12	1
SEPTEMBER	20	20	5
OCTOBER	23	24	5
NOVEMBER	24	25	13
DECEMBER	22	23	7
TOTAL	109	112	34

Note: 3 sets of twins were reported as stillbirths.

TABLE 2.9: Misclassification errors and their effect on cohort mortality rates: April,1981 - December, 1981 birth cohort: EMIS Bobodioulasso.

		Corrected	Observed	Corrected / Observed
STILLBIRTHS		167	240	0.696
DEATHS RECORDED AT:	1 MONTH	203	130	1.562
	1 YEAR	483	410	1.178
	2 YEARS	636	563	1.130
POPULATION AT RISK* DURING:	1st MONTH	5477	5404	1.014
	1st YEAR	5059	4986	1.015
	2 YEARS	4874	4801	1.015
PROBABILITY OF DYING WITHIN:	1 MONTH	0.0371	0.0241	1.541
	1 YEAR	0.0955	0.0822	1.161
	2 YEARS	0.1305	0.1173	1.113

* Children under observation at the end of the reference period plus observed deaths: losses to follow-up are excluded.

Erroneous stillbirths are births who are diagnosed by the nurse as having given a sign of life but are reported as stillbirths by the woman. Table 2.9 shows that the confusion of early child deaths with stillbirths would affect quite seriously the cohort's mortality indicators. The measure the most sensitive to such errors is the neonatal mortality rate: it would be underestimated by about 54% ! The infant mortality rate (q(1)) and q(2) would be underestimated by 16% and 11% respectively. This means that in the absence of recall errors the measurement of mortality may still be seriously affected by another major type of error, namely the **confusion of early child deaths with stillbirths**, or **misclassification errors**.

It is very interesting to note that most of the erroneous stillbirths were diagnosed by the nurse as having been "reanimated in vain". In such cases the mother may even be unaware that she had a live birth. It should also be noted that no stillbirth was reported erroneously as a live birth who died. In other words, in the follow-up survey, misclassification went only in one direction.

2.6 - **Reliability of aggregate retrospective indicators.**

Our data do not allow the quantification of exactly how errors in B, S and D affect the retrospective measures. In this section we are mainly interested in investigating how the means of the key variables (B, S, D) and the proportions dead behave in the presence of the errors noted in the preceding sections. The means by age group are given in Table 2.10 and graphed in Figure 2.1.

Despite individual inconsistencies, the means are quite stable. The difference between sources is not significant for any of the age groups. This is due to the fact that, in retrospective data, individual errors tend to compensate for each other.

The proportions deceased are given in Table 2.11 and graphed in Figure 2.2. For almost all age groups, the proportions are higher for the registers than for the survey. The difference between sources is higher for younger women and decreases steadily up to age 35. This suggests that omissions due to recall lapse, would which have a marked effect only for older women, do not constitute a major factor in the reliability of the proportions dead, the raw material for the study of mortality.

TABLE 2.10: Mean number of events by age group and source.

AGE GROUP	Ever born R	Ever born S	Surviving R	Surviving S	Deceased R	Deceased S
15-19	0.26	0.25	0.18	0.18	0.08	0.07
20-24	1.50	1.46	1.15	1.15	0.35	0.31
25-29	3.52	3.54	2.76	2.80	0.76	0.74
30-34	5.38	5.43	4.21	4.27	1.17	1.16
35-39	6.99	7.16	5.56	5.66	1.43	1.49
40-44	8.50	8.45	6.50	6.53	2.00	1.93
45-49	9.50	9.50	8.25	8.25	1.25	1.25
ALL	2.72	2.73	2.12	2.15	0.60	0.58

TABLE 2.11: Proportions deceased by source and age group of mothers.

AGE GROUP	PROPORTIONS DECEASED Register	Survey	R-S
15-19	0.2959	0.2671	0.0288
20-24	0.2306	0.2153	0.0153
25-29	0.2164	0.2109	0.0055
30-34	0.2172	0.2133	0.0039
35-39	0.2045	0.2089	-0.0044
40-44	0.2353	0.2278	0.0075
45-49	0.1316	0.1316	0.0000
ALL	0.2189	0.2133	0.0055

FGURE 2.1: Mean number of children ever born (B) and
children surviving (S) by source: Bobo 1981–82

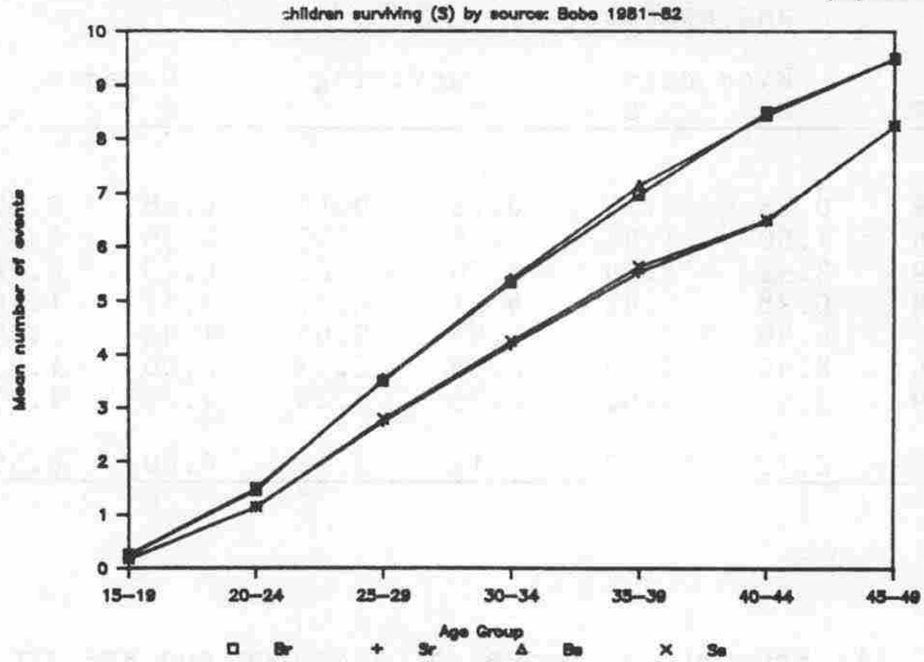
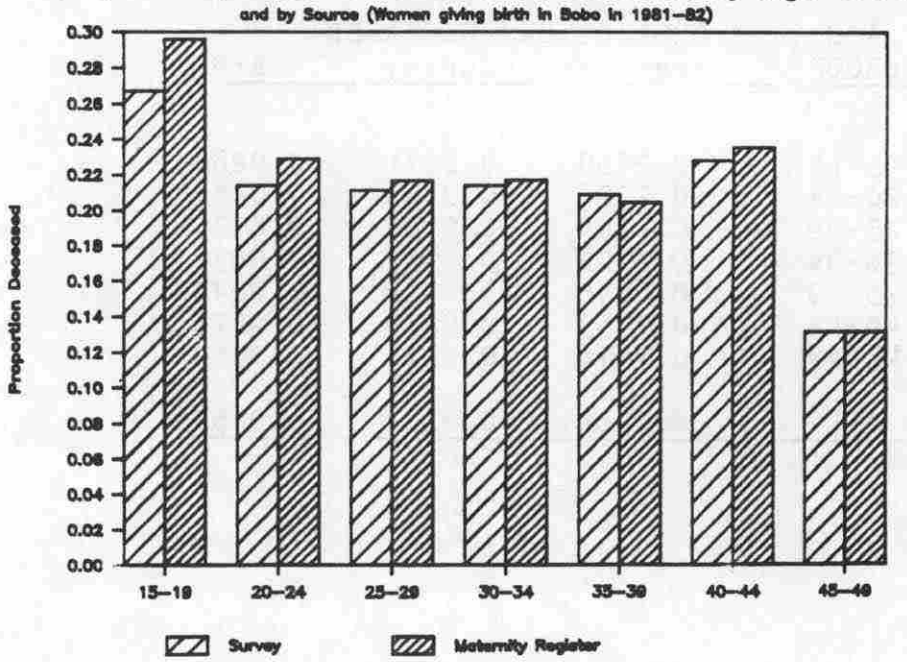


FIG 2.2: Proportion of Deceased Children by Age Group
and by Source (Women giving birth in Bobo in 1981–82)



2.7- Conclusion.

In this chapter, we have compared the reports of the same mothers to two different inquiries. Despite the fact that these inquiries were based on different approaches, and despite the difference in the levels of completeness, the results are similar - the aggregate retrospective measures (means and proportions dead) are not very different. The mean numbers of intrauterine deaths are the only exception (see Table 2.1). Thus non-live births are more poorly reported than live births. Reporting of deaths is less consistent than reporting of surviving children. Inconsistencies in the reporting of the number of children ever born are mainly due to inconsistencies in the reporting of live births who died. There are probably almost as many reasons for omitting child deaths as there are respondents. However, our data suggest three major determinants of the consistency of reports on children who died and subsequently of the consistency of reports on live births: the interviewer, the number of events to be reported, and misclassification errors.

Interviewers differ in their understanding and their implementation of the survey definitions and principles. It has been extensively documented that the quality of the questions determines the quality of the responses (Quandt, 1973; Gibril, 1974; Thompson et al., 1982). However, even if the formulation of questions is perfect some errors will be unavoidable; the respondent may, for one reason or another, voluntarily give a wrong answer - for example when she learns during the interview that, by reporting a stillbirth she will avoid the harassment of having to answer other unwelcome questions. It should also be noted that most interviewer errors are on the number of children who died. There is no significant difference between interviewers as far as the recording of the number of living children is concerned.

Misclassification errors, the most important factor, seem to be due both to interviewers and respondents. A significant number of live births who died may be omitted simply because the mother does not see the need to report them as such or because the interviewer asked the wrong question. It is very unlikely that this type of errors has anything to do with recall lapse. Even in the follow-up survey where the length of the retrospective period is limited to a few hours, 56% of the neonatal deaths and 18% of the infant deaths were wrongly recorded as stillbirths.

The cumulative number of events to be reported is the third factor; it has a relatively lower impact than the two first ones. The higher the number of events to be reported, the farther back in time those events have occurred, and the higher the probability of misreporting them. It should however be noted that, because of the exclusion of age from the regression, the effect estimated for the number of events is exaggerated: problem of multicollinearity.

Finally the quality of reports does not seem to depend much on socioeconomic factors. Because the mean numbers of events and the proportions dead are not significantly different between sources, the hospital records can therefore be substituted for the survey data with no major changes in the estimates.

CHAPTER 3

ESTIMATION OF CHILD MORTALITY USING EXISTING TECHNIQUES.

3.1- Introduction.

Brass's technique for estimating mortality (Brass and Coale, 1968) and its later versions (Sullivan, 1972; Trussell, 1975) have made available useful infant and child mortality measures for many developing countries despite the absence of dependable civil registration systems. The technique transforms the proportion of deceased children, for each age group of women, into probabilities of dying before specific ages. The proportion deceased among children ever born to a given subgroup of women will be a function of the mortality level and the durations over which the children were exposed to the risk of dying. For a given group, the proportion deceased is a weighted average of the life table probabilities of dying applicable to their children. The weights express the distribution of exposure time. However this robust technique is not applicable to data from conditional samples e.g. samples of women having a birth within a definite period of time.

The Surviving Children Technique (Preston and Palloni, 1978) is theoretically applicable to both conditional and unconditional samples. Instead of estimating the time distribution of children ever born as in the Brass technique, the Surviving-Children Technique involves reverse-surviving the surviving children using their current age distribution. The authors point out the interest of applying the technique to the last child only. However, the age distribution recorded by EMIS Bobodioulasso is not precise enough for a useful application of the technique. We do not have dates of birth but only age in completed years: 0,1,2,...10. More precision is needed mainly in the first 2 years of life where the q(a) function changes rapidly. A methodology applicable to conditional sample data is therefore needed.

Brass (1971) first considered using such data to estimate infant and childhood mortality in the Solomon Islands. The approach he suggested constituted the basis for future developments in methodology by McCrae (1982). Since then, as knowledge of the problems relating to the analysis of conditional sample data has improved, and as the need for more reliable data has become clearer, different approaches have been proposed (see Brass and McCrae, 1984, 1985; and Fargues, 1985). The Preceding Birth Technique (Brass and McCrae, 1984) uses information on the survivorship of the child preceding the one that is born at time of interview to estimate a single index of the life table: q(2). The two other techniques use data on the survivorship of all previous births. The Adapted Multiplying Factor Technique (Brass and McCrae, 1985) is based on a hypothetical reconstruction of the unconditional from the conditional proportions deceased.

Fargues (1985a) uses a different approach. He uses the observed age and parity distribution to estimate the average duration of exposure to the risk of mortality for children born to each age group of mothers. In this chapter, the three techniques are applied to the EMIS Bobodioulasso data and a critical evaluation attempted.

3.2- Use of data on the survivorship of the preceding child: the Preceding Birth Technique (PBT).

Brass and McCrae (1984) propose a way of estimating mortality applicable to data from conditional samples. The Preceding Birth Technique (PBT) requires that one simple question be asked to women giving birth: "Is your preceding child alive ?"

The proportion deceased among preceding births, all mother's ages combined, π, was suggested as an acceptable estimate of q(I), where I is the mean length of the interbirth interval (Brass, 1971). But the non-linearity of the mortality function in infancy and early childhood makes π equal to q(I*) where I* is less than I. I* depends on the age pattern of mortality and the birth interval distribution (which expresses the distribution of exposure). However, because of the already noted relative over-exposure of the children to the risk of dying (relative to those reported in unconditional samples), I^* is not very sensitive to differences in either birth interval distributions or mortality patterns. π is therefore a consistent index of mortality over populations. Brass and McCrae estimate that $I^* = \alpha I$ where α is a constant, is a good approximation of the relationship between I and I^*. Thus, applying Brass' African and General Standard life tables, they determine that a value of α of 0.8 is satisfactory[1]. If the mean length of the birth interval is close to 2.5 years, then:

$$I^* = 0.8 \times 2.5 = 2 \text{ years and } \pi = q(2).$$

The technique is expected to give a reliable estimate of q(2) for at least 3 reasons:

i) It is based only on the most recent mortality experience. Reporting errors, which tend to increase the longer that the recall period extends back in time and the higher the number of events to be reported, are therefore minimal;

ii) It amalgamates the experience of mothers of all ages while usually, estimates of q(2) are based exclusively on reports of younger mothers, which do not include high

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[1] In addition to Coale and Demeny (1966) Model life tables, I used the two Senegalese life tables of Pison (1982) and Garenne (1982) to simulate π for different birth interval distributions. The relationship $\pi \approx 0.8I$ holds for all these life tables.

order births;

iii) It is not dependent on model life tables.

However, because it is based on the experience of only one child, the estimate provided by the technique may be very sensitive to under reporting of early child deaths. In addition, in populations where the mean length of the interbirth interval is very different from 2.5 the technique will yield a biased estimate of q(2). According to the authors, the fact that last births are excluded does not seriously affect the estimate.

This technique is not directly applicable to EMIS data but fortunately it can be adapted. The question asked in these surveys is slightly different from the one actually required by the technique. Instead of asking:" What happened to your preceding live birth?" the EMIS asked:" What is the outcome of your preceding pregnancy?" The allowable answers to this question are:

1- live birth, child still alive;

2- live birth, child dead since;

3- spontaneous abortion;

4- stillbirth;

5- this is the first pregnancy.

Call the proportion deceased among live born computed from this question g. To adapt the PBT to our data we need to find the relationship linking π to g. The measure g is different from π because for some mothers the preceding pregnancy ended in an intrauterine death instead of in a live birth (see Table 3.1).

For the 369 mothers who had an intrauterine death, we would need to go back upstream at least one more pregnancy to get to the preceding live birth. The live births included in the calculation of g have therefore been exposed to the risk of dying for a shorter time than those included in the calculation of π. In other words preceding children born to women with a longer last birth interval are excluded. Let the average exposure time of live births originating from the preceding pregnancy be I_g. I_g is the interval between deliveries as opposed to I which is the interval between live births. Ig is therefore less than I, and the difference between the two is the amount by which pregnancy wastage increases the average birth interval. For example if I = 30 months and if fetal wastage increased the birth interval by an average of 2 months, then we would have I_g = 28 months (2.33 years) and

$g = q(0.8I_g) = q(1.86) = 0.1158$. However, this approximation procedure is not attractive for at least two reasons:

i) we do not know by how much fetal wastage increments the birth interval in our population. The average estimated by Bongaarts (1982) is about 1.5 months; however this may not be correct for the population under study.

ii) for comparison purposes it is more convenient to have the probability of dying by an exact age, for example q(2). Even if we knew exactly I_g, it would be difficult to go from $q(0.8I_g)$ to q(2). The relationship between the two would be a function of the age pattern of mortality which is a major unknown.

The data presented in Table 3.1 allow us to give a gross range of variation for π and therefore for q(2). Because Ig is less than I and q(a) is monotonic increasing in the relevant range of ages, we know that π is greater than 0.1158 the proportion deceased among preceding pregnancy ending in a live birth. We also know that π is less than 0.1822, the proportion dead that would be obtained if all 369 intrauterine deaths - stillbirths and abortions - were considered as consisting of live births who died, (in other words if the rate of intrauterine mortality were equal to 0). We can therefore write: $0.1158 < \pi < 0.1822$. It should be noted here that the Adapted Multiplying Technique estimate of q(2) is 0.1856.

Now consider women who have reported only one previous live birth (2nd Column of Table 3.1). The proportion dead among the children ever born to these women, d_1, is also the proportion dead among the preceding live births, $\pi_1 = 0.1608$. The proportion dead computed using the question on the outcome of the preceding pregnancy, g_1, is equal to 191/1361 = 0.1403. If the structure of the birth interval is not parity related - the effect of fetal loss on the birth interval does not vary with parity - then g_1 and π_1 will be related by the same relationship that links g to π. Then it will be the case that:

$$\frac{\pi_1}{\pi} = \frac{g_1}{g}$$

Knowing π_1, g_1 and g, we can estimate that:

$$q(2) \approx \pi = \pi_1 \times g/g_1 = 0.1608 \times 0.1158/0.1403 = 0.1327.$$

What is this adaptation of the Preceding Birth technique worth? The results of the Bobodioulasso follow-up survey allow a tentative answer to this question. The q(2) estimated here is not directly comparable to the one given by the follow-up survey because the two probabilities do not pertain to either the same time period or the same population. The problem arises because the women lost to follow-up report a higher risk of dying, as indicated by Table 3.2.

The major limitation of the Preceding Birth Technique is that it can give no indication about the age pattern of mortality. To get an idea about the age pattern, estimates of the probability of dying by at least two ages , for example q(2) and q(5),are needed (under constant mortality) If the comparison is limited to women who were not lost to follow-up, the Adapted Preceding Birth Technique estimate is virtually identical to the follow-up survey estimate: 0.1269 versus 0.1251. The difference between the two is only about 1%. It should be noted that the follow-up q(2) estimated for children who were not lost to follow-up is overestimated. This is because all losses to follow-up are excluded altogether while some of them were under observation for a while. Note also that this estimate pertains to about mid-1983 while the PBT estimate pertains approximately to 1980.

However, this limitation is more than counterbalanced by the strength of the technique and the ease of its application. Thanks to this approach, mortality trends in many developing countries can be monitored more easily. The existing administrative circuits - civil registration and health care systems - may be used for this purpose with little additional cost. The results of the Malian experience (Hill et al., 1985) will be instructive in this aspect.

Table 3.1: Outcome of the preceding pregnancy of women giving birth between April 1, 1981 and March 31, 1982 in Bobodioulasso.

Previous Outcome	All women	Women with only one previous live birth
Live birth	6051	1361
alive	5350	1170
dead	701	191
Intrauterine death	369	71
Number of cases	6420	1432
Proportion dead Among live births	0.1158	0.1403
	g	g_1

Table 3.2: Comparison of mortality indicators for women still under observation at the end of the follow-up survey and those who were lost to follow-up (EMIS Bobodioulasso).

	Number of women	g	π	Follow-up q(2)
ALL WOMEN	8343	0.1158	0.1327	-
NOT LOST TO FOLLOW-UP*	6570	0.1102	0.1269	0.1251**
LOST TO FOLLOW-UP	1773	0.1409	0.1592	-

* child under observation at 7th round or death observed.
** number dead by age 2 divided by initial population.

3.3 Use of data on the survivorship of all previous births

Unlike the Preceding Birth Technique, techniques using data on the survivorship of all previous births require knowledge of age, number of children ever born and number surviving for each respondent.

3.3.1 The Adapted Multiplying Factor Technique(AMFT).

The AMFT estimates mortality from the proportions dead by age group of women. Brass (1971) first suggested that his original technique could be adapted to the conditional case if allowance were made for the longer-than-average (average in the unconditional case) exposure to mortality. His reasoning was based on the fact that at any given time the mothers in a population are randomly distributed over the last birth interval and not concentrated at the end as is the case here. The traditional technique may be applied to retrospective data collected at the time of a birth in the usual manner; but the probabilities will refer to ages half a birth interval older. With a birth interval of 2.5 years, the first age group will yield q(1+1.25) or q(2.25) instead of q(1). In a similar way the group 20-24 will give q(3.25) and so on. Afterwards, the conventional measures, q(1), q(2), etc., can be determined by interpolation using the logit system (p. 192). However, because of the non-linearity of mortality this attractive solution tends to overestimate mortality (McCrae, 1982). McCrae (1982) introduced an alternative way of accounting for the longer exposure. This consists of adding a proportion of the current births (even if none of them have as yet been exposed to mortality) to previous ones to obtain the denominator used in the calculation of the proportions dead. The approach was refined later to give what is known today as the Adapted Multiplying Factor Technique. The AMFT is essentially based on an approximation of the relationship between the proportions deceased in the conditional and unconditional cases.

The reasoning behind the technique can be summarized the following way. Let d(c) be the proportion dead for a given age group of women in the conditional sample. Then

$$d(c) = \frac{D}{PB} = \frac{\text{Number of deaths among previous births}}{\text{Number of previous births}}$$

To simulate the unconditional case, the women are considered to have given birth all at the same time. The date of interview corresponds therefore to the middle of the reference period. If B is the number of current births, then in addition to the D previous ones, B/2 births would have been observed. And if these B/2 births were then submitted to the same life table as the one experienced by the previous ones, then De additional deaths would have been observed. The proportion deceased among the B/2 births would be d(e) = De/(B/2). The overall proportion dead in the unconditional case would be: d(u) = (D+De)/(PB+B/2) Which can be written:

$$d(u) = \frac{D}{PB+\frac{1}{2}B[1-d(e)/d(u)]}$$

Brass and McCrae consider that d(e)/d(u) may be replaced by a constant. Dividing both numerator and denominator by PB they have:

$$d(u) = \frac{D/PB}{1+k(B/PB)} \qquad \text{where } k = \tfrac{1}{2}[1-d(e)/d(u)]$$

They assume a value of 0.2 for k. Then assuming that "the distribution [of previous births] for women recording a birth is much the same as for women surveyed at random" (p.7), they consider that P_2/P_3 for the whole population or the one calculated from the conditional case (after redistribution of half the current births) may be used as a fertility location parameter.

The authors recommend concentration on the age groups 20-24 and 25-29 and therefore on q(2) and q(3). These "preferred" estimates are unreasonably high for Bobodioulasso. For example the technique gives a q(2) of 0.1856 (see Annex I) which is out of the range of possible values implied by the Preceding Birth Technique and the prospective data. In fact, for the first three age groups, a potential source of error which may be very critical is the replacement of d(e)/d(u) by a constant. As recognized by the authors, the value of d(e)/d(u) depends on the birth interval distribution, the mortality pattern, and the age group. Because current births represent a relatively high proportion of all births, estimates derived from these age groups will be sensitive to errors implied by the value assumed for $k = \frac{1}{2}[1-d(e)/d(u)]$. The best estimate given by the AMFT - the one most consistent with estimates from other techniques - is q(5) estimated from reports given by age group 30-34, which is about 210 per thousand live births.

An additional source of error may emerge from the underlying assumption that children born to different groups of women have experienced the same level of mortality. In other words mortality is assumed to be homogenous. (In the following this is referred to as the assumption of homogeneity). As is the case for the first age group, the mortality rates experienced by children born to women 20-24 are above the average not only because most of these are first births, but also many of them occurred when the mothers were teenagers. In Bobodioulasso, the proportion deceased among preceding births is higher than the average, only for the first 2 age groups of women (Annex II). In this case estimates based on these groups will be too high.

As clearly shown by Fargues (1985a; 1986), the AMFT underestimates mortality at high ages, 10 and above for example. As noted in Section 1.4, after age 35 of the women, children reported in conditional samples have been exposed to mortality for a shorter time than those in unconditional samples, nevertheless, the correction proposed by Brass and McCrae always leads to lower unconditional proportions deceased and therefore to an underestimation of q(10) q(15) and q(20). It is probably for that reason that the authors do not recommend use of reports of women over 35 years of age.

3.3.2- Fargues's approach

Instead of correcting the proportions deceased to make them correspond to q(a) for a=1, 2, 3, ..., Fargues (1985) determines the exact ages to which the proportions pertain under the assumption of constant mortality. In other words, for each age group x of mothers, a_x is determined such that $d_x = q(a_x)$. When mothers are interviewed at the time of childbirth the exposure of previous births is a direct function of the mean length of the interbirth interval. Consider the W(x,n) women aged x who report n previous births. If the birth interval has remained constant and equal to In and if mortality was unchanged in recent years, then we would have the following correspondence:

Birth rank	Proportion deceased
n	$q(0.8I_n)$
n-1	$q(2I_n)$
n-2	$q(3I_n)$
.	.
.	.
1	$q(nI_n)$

Where the $0.8I_n$ comes from the assumption made by Brass and McCrae in the Preceding Birth Technique (Brass and McCrae, 1984).

The mean number of children who died for one women would be

$$\delta_n = q(0.8I_n)+q(2I_n)+q(3I_n)+.....+q(nI_n)$$

The overall proportion deceased for women age x would be:

$$d_x = \frac{\sum_n W(x,n)\delta_n}{\sum_n nW(x,n)} \quad , n = 1, ..., \infty$$

For application to EMIS Bobodioulasso, the value of 2.5 years is assumed for the mean length of the birth interval, Ibar. This same value is used for each parity group. Knowing W(x,n) and I_n=Ibar, d_x may be calculated by adopting a model life table. The hypothetical $d_x{}^s$ is then situated in the set of $q^s(a)$ of the standard and a_x is determined by interpolation such that $d_x{}^s = q^s(a_x)$. Finally the observed d_x is taken as equal to $q(a_x)$, the probability of dying by age a_x in the life table applicable to the children. If necessary the probability of dying by exact ages 4, 5, etc. may be determined by interpolation. Because of the increasing proportion of women excluded at older ages, Fargues recommends that reports of women 40 and above be disregarded. The author also recommends that the proportion calculated for the age group 15-19 be disregarded because of the higher mortality risk of children born to teenage mothers. However, this does not entirely solve the problem of non-homogeneous mortality: children born to women 20-24 have also experienced higher death rates (Annex II). In the presence of heterogeneity, any measure based on any single group may be distorted. The extent of the error depends on how different from the average the mortality of the selected group is. For example, probably because of the high mortality of children born to mothers aged 20-24, the proportion dead for this age group is almost the same as the one calculated for the age group 25-29: 0.2085 versus 0.2094. (Table 3.3) As the q(a)'s for exact ages are determined by interpolation between the observed proportions deceased, they will be affected by variations in the level of mortality of children born to different age groups of women. In the presence of differential errors or non-homogeneous mortality experience, the crude proportions must be adjusted in order to make the estimated q(a)'s consistent.

Fargues shows that:

i) a_x is determined mainly by the length of the birth interval. It is not sensitive to the age and parity distribution and therefore to the fertility pattern.

ii) a_x is not very sensitive to the assumed standard and therefore to the age pattern of mortality. The only exception arises with the Peul Band life table (Pison,1982). According to the author, this exception is due to the peculiarity of mortality in the villages studied by Pison(1982). This then is the rationale for concluding that the age pattern of mortality has no effect on the estimates. In later writings, reference to this exception is omitted altogether. In his study of infant and child mortality in Abidjan, we read: "We have shown elsewhere [Fargues,1985a] that these ages [a_x] do not depend on the mortality model used. Those estimated using Brass' African Standard are therefore equal to those that would have been found were the real life table applicable to the children of Adjam used as a standard, life table which is unknown a priori" (Fargues, 1985b:p4).

The Bandafassi pattern which he states is peculiar has been observed throughout Senegal and in rural Gambia (Cantrelle, 1969, 1974; McGregor and Williams 1979; Pison and Langaney, 1985; Garenne 1981, 1982). The major characteristic of this "Senegambian" age pattern is an excessively high child mortality, which translates into a ratio of infant to child mortality (${}_4q_1/{}_1q_0$) far greater than 1. It is mainly because of its high death rates between ages 1 and 5 that such a life table yields higher estimates than the Princeton tables (Coale and Demeny, 1966) and Brass's African Standard (Brass and Coale, 1968). The value of $_4q_1/_1q_0$ implied by the Peul Band life table is 1.36. Because of the doubts expressed by Pison about the reliability of this table (because of small numbers of events) we did not use it in our calculations. Instead, we used the Bandafassi life table which aggregates the preceding one with observations in neighboring populations (Pison and Langaney, 1985). In this table, $_4q_1/_1q_0$ = 1.28. Table 3.3 gives the results of applying the method to the Bobodioulasso data.

Table 3.3- Application of Fargues' approach to EMIS Bobodioulasso

Age of Women x	Proportion deceased dx	Model Life Table African Std ax	Model Life Table Bandafassi ax
15-19	.2585	2.40	2.35
20-24	.2085	3.20	2.92
25-29	.2094	5.15	4.08
30-34	.2190	7.14	5.29
35-39	.2231	8.38	6.58
40-44	.2568	8.74	6.87
Age of Children a		**q(a)**	**q(a)**
3	---	---	.209
4	---	.209	.209
5	---	.209	.217
6	---	.213	.221
7	---	.218	---
8	---	.222	---

Despite the fact that the Bandafassi life table is but a "mild" expression of a more pervasive pattern, when used as a model, it still yields estimates which are noticeably higher than those derived from the African Standard: 217 versus 209 per 1000 for q(5). Use of the Ngayokheme life table would have given even higher estimates. Fargues' contention would hold only if one could demonstrate that this pattern cannot exist in the areas where the EMIS surveys were conducted. Note that Cantrelle et al. (1969) and Cantrelle and Garenne (1985) have documented its prevalence in the very area where EMIS Senegal was conducted. (It should be noted, however, that these data were collected more than 10 years ago and that the situation may have changed since then). We therefore think that, as far as the analysis of the EMIS surveys is concerned, the age pattern of mortality does make a difference.

3.3- Discussion

The retrospective estimate of q(2) based on all previous births should be consistent with the Adapted Preceding Birth Technique (APBT) estimate of q(2) since the preceding children represent a non-negligible proportion of previous births (about 26%). This consistency is not the case for the Adapted Multiplying Factor Technique which q(2) estimate, 0.186, is out of the range of possible values implied by the Preceding Birth Technique. The observed difference is 53 per 1000 (186-133). Because the time location of both estimates is almost the same (Brass and McCrae, 1985), this large difference cannot be explained by mortality trends. One or many of the assumptions made by either technique or both techniques are probably violated. The most critical assumption is the assumption of homogenous mortality made by the AMFT, which postulates that all previous births have experienced the same death rates. Annex II shows that children born to different age groups of women have experienced quite different death rates, questioning hence the assumption of homogeneous mortality. The proportions deceased for the first two age groups are not comparable with reported by the other age groups. Even if mortality were not dependent on the age of the mother per se, the assumption of homogeneous mortality would still be violated because of the noted selection of younger women who lost their preceding child (see Chapter 1; Section 1.4). Note that because of the higher mortality of children reported by the first two age groups, Fargues' approach cannot give reasonable estimates of either q(1), q(2) or even q(3) without coorection for non-homogeneity.

To summarize, let us say that the Adapted Multiplying Factor Technique and Fargues' approach suffer mainly from the assumption, made by both techniques, that mortality is homogeneous. As we have seen in Chapter 1, there is a selection of young women for a higher mortality of their preceding children. With parity grouping this selection bias is relatively diluted by the fact that, low parity groups do not include exclusively younger women for whom the selection is very accentuated.

Partly because the proportions deceased by age groups are not comparable, any trends estimated from a comparison of the raw proportions would be biased. Fargues ignores entirely the effect that changing mortality will have on his estimates and does not mention any way of estimating trends. Brass and McCrae (1985) use the single estimate of q(2) given by age group 20-24 in successive years to estimate trends. That procedure is not possible with EMIS data: they constitute a single point observation. A technique allowing the estimation of mortality trends is therefore needed.

CHAPTER 4

DEVELOPMENT AND TESTING OF AN ALTERNATIVE APPROACH

4.1. Introduction

The indirect estimation of mortality consists of transforming proportions deceased among children born to different subgroups of mothers into probabilities of dying by specific ages. So far, only age and marital duration have been used as grouping criteria. In their surviving-children technique, Preston and Palloni (1978) suggest that parity might also be used for unconditional samples:

> "Parity is a grouping criterion that deserves but has not received attention. It has the advantage over marital duration in that backward movement from state to state is impossible, as well as some possible advantages over age in that child mortality may covary less with the status itself and in that the numbers of events observed in the various reporting classes may be more precisely equal".(p.74)

Probably the major issue in deciding the choice of a grouping criterion is how well it controls for exposure time. In the analysis of data from unconditional samples, age and marital duration have proved their worth. Parity has never been used. But with data from conditional samples, parity grouping is more appropriate. In unconditional samples, for example, parity grouping will not distinguish a 50 year old women who had her only child when she was 15 from a 15 year old women who just had her first child. In the same group - parity 1 - exposure time would vary between 0 and 35 years. When mothers are interviewed at the time of childbirth, parity grouping does not allow such large variations in exposure time within the same group. All women who stopped childbearing, including the 50 year old woman, would have been excluded. The range of variation of exposure time is narrowed down to the range of variation of the interbirth interval. The age or duration of exposure of previous births is a direct function of the mean length of the birth interval. This was noted by Fargues (1985a). However, after having estimated exposure time using the mean length of the birth interval, he returned to age as a grouping criterion.

Whatever the grouping criterion used, the following identity holds for each group (Preston and Palloni, 1978: p.72):

$$d = (D/B) = (1/B) \int_0^A B(a)q(a) = \int_0^A c(a)q(a) \qquad (1)$$

q(a) = Proportion deceased among children born a years earlier,

d = proportion deceased among children ever born.

B = total number of children ever born to reporting women,

D = number deceased among these children,

c(a) = proportion born a years ago,

A = years elapsed since the birth of the first child.

Knowing d we need an approximation of c(a) - the distribution of exposure time - to be able to infer q(a). With conditional samples, the estimation of exposure time is a fairly straightforward matter when parity is used as grouping criterion.

In this chapter a new technique is proposed which differs from the preceding ones mainly in the grouping criterion. Mothers are grouped by parity instead of by age. This procedure allows us first of all to drop the critical assumption that all children have experienced the same death rates: assumption of homogenous mortality. Two cases are distinguished: constant mortality (Section 4.2) and changing mortality (Section 4.3). When mortality has been constant, the assumption of a standard mortality pattern is unnecessary for the estimation of the set of probabilities of dying. If mortality has been changing the life table estimated under the assumption of constancy reflects both the age pattern and trends in mortality. The assumption of an age pattern becomes necessary for one to be able to estimate recent mortality trends. Finally the technique is tested on U.S. data (Section 4.5).

4.2- **Solution under constant mortality**

If mortality has remained constant during the 10 to 15 years preceding the survey, the set of q(a)'s estimated using reports of different parity groups will pertain to the current life table. There are three versions of the new technique. All three models produce a set of estimates under the assumption of constancy. The first two (ITER and GEOM) do so without assuming a standard mortality pattern. ITER can be estimated when the age distribution of preceding children is available, it uses an iteration procedure. GEOM does not require knowledge of that age distribution, it uses the geometric mean of individual children's exposure time to estimate the reference age -- age a_n such that d_n=q(a_n) -- for each parity group. The third one (MLTA) is based on the assumption of a model life table. The procedure for estimating mortality trends presented in Section 4.3 is derived from MLTA. It should be noted that the first two models can also be used to estimate mortality trends by comparing the set of q(a)'s to a model life table.

4.2.1- **Age of the preceding is available (ITER).**

When the age distribution of preceding children is available, it can be used for a quite precise estimation of Equation 1.

The notation is as follows:

W_n = number of women reporting n previous births.

B_n = number of children ever born to these women.

Dn = number dead among these children.

d_n = proportion dead = D_n/B_n.

e_n = mean exposure time of the B_n children.

I_n = mean length of interbirth interval for the W_n women.

If A_n is the number of years since the birth of the first child and $c_n(a)$ the proportion of children born a years ago, the, we can write:

$$d_n = \int_0^{A_n} c_n(a)q(a)da \qquad (2)$$

Taylor's expansion gives:

$$q(a) = q(e_n)+q'(e_n)(a-e_n)+\tfrac{1}{2}q''(e_n)(a-e_n)^2+...$$

Neglecting terms of order 3 and above and replacing this value of q(a) into equation (2) gives:

$$d_n = \int c_n(a)q(e_n)+\int c_n(a)q'(e_n)(a-e_n)+\tfrac{1}{2}\int c_n(a)q''(e_n)(a-e_n)^2$$

$$= q(e_n)+0+\tfrac{1}{2}q''(e_n)V_n(a)$$

And finally we have:

$$d_n = q(e_n)+\tfrac{1}{2}q''(e_n)V_n(a) \qquad (3)$$

$V_n(a)$ is the variance of ages of children ever born to women reporting n previous births, and $q'(e_n)$ and $q''(e_n)$ the first and second order derivatives of the function q(a) at e_n. a_n and $V_n(a)$ are functions of the age-parity specific fertility schedule. q" represents the curvature of the q(a) function and therefore reflects the age pattern of mortality and mortality trends.

Omitting women reporting 10 or more previous births, we are left with the following set of 9 equations.

$$d_1 = q(e_1) + \tfrac{1}{2}q''(e_1) \times V_1(a) \qquad (i)$$
$$d_2 = q(e_2) + \tfrac{1}{2}q''(e_2) \times V_2(a) \qquad (ii)$$
$$\vdots$$
$$d_9 = q(e_9) + \tfrac{1}{2}q''(e_9) \times V_9(a) \qquad (ix)$$

Knowing d_n (observed) and e_n (see estimation of e_n below), a solution to this equation is any combination of one q(a) function and one set of $V_n(a)$'s for n=1 to 9 that satisfies the 9 equations simultaneously. There is an infinity of such solutions. We can attempt to find one of these solutions only when we can estimate $V_n(a)$ for n=1, 2, .., 9.

Estimation of e_n and $V_n(a)$.

Consider women reporting n previous births. Children of order i born to these women were born on average iI_n years before the survey date, where I_n is the mean length of the interbirth interval. All B_n previous births were born on average e_n years ago with:

$$e_n = [1/n]\sum iI_n \qquad \text{where } i = 1, 2, \ldots, n$$

$\sum i$ for i=1 to n is equal to $\frac{1}{2}(n+1)n$ and we have:

$$e_n = (n+1)I_{n/2} \qquad (4)$$

This relationship is in fact based on the logic used by Fargues (1985a). Assuming that the previous birth to woman w were separated on average by a duration equivalent to the mean length of her birth interval I_w, such that $I_w = I_n + \varepsilon_w$ where ε_w is her deviation from the mean, then we can estimate that child order i of this woman was born approximately $e_{i,w}$ years ago with: $e_{i,w} = iI_n + i\varepsilon_w$

The variance of the age distribution of all previous births is simply the mean sum of square deviations of $e_{i,w}$ from the mean, a_n. We can therefore write:

$$V_n(a) = (1/nW_n)\sum_{iw}\sum (e_{i,w}-a_n)^2 = (1/nW_n)\sum_{iw}\sum (iI_n+i\varepsilon_w-a_n)^2$$

$$= (1/nW_n)\sum_{iw}\sum [(iI_n-a_n)+i\varepsilon_w]^2$$

$$= (1/nW_n)\{\sum_{iw}\sum (iI_n-a_n)^2 + \sum_{iw}\sum (i\varepsilon_w)^2 - 2\sum_{iw}\sum (iI_n-a_n)i\varepsilon_w\}$$

$$= (1/nW_n)\{\sum_{iw}\sum (iI_n-a_n)^2 + \sum_i i^2 \sum_w \varepsilon_w{}^2 - 2\sum_i (iI_n-an)i\sum_w \varepsilon_w\}$$

$$= (1/n)\sum_i (iI_n-a_n)^2(1/W_n)\sum_w 1 + (1/n)\sum_i i^2V_n(I) + 0$$

$$= (1/n)\sum_i (iI_n-a_n)^2 + (1/n)\sum_i i^2V_n(I)$$

where $V_n(I) = (1/W_n)\sum_w (I_w-I_n)^2 = (1/W_n)\sum \varepsilon_w{}^2$

At the end we have:

$$V_n(a) = (1/n)\sum_i (iI_n-a_n)^2+\{V_n(a)/n\}\sum_i i^2 \qquad (5)$$

Equation (5) shows that the total variance is the sum of two terms which represent respectively the variance within individual women and the variance across women. The only unknown in this equation is $V_n(I)$, the variance of the birth interval across women. To estimate $V_n(a)$ we therefore need to first estimate $V_n(I)$.

4.2.2- Estimation of $V_n(I)$

When the age distribution of the preceding children is available, it can be used to estimate the variance of the interbirth interval across women. The age of the preceding child represents the length of the last closed birth interval for a given mother.

If z is the age of the preceding child then we have $V_n(I)=V_n(z)$. The mean age Zbar, will also be equal to I_n for women reporting n previous births. This implies that:

$$V_n(I)=V_n(z)=\sum c(z)(z-I_n)^2$$

where $c(z)=B(z)/\sum B(z)$ represents the proportion aged z where B(z) is the number of births z years ago. However, our data only gives the age of the S(z) surviving children.

The number of births z years ago is obtained by reverse surviving these children:

$$B(z)=S(z)/[1-q(z)]$$

The first estimate of q(z) yielded by ITER is used to start with.

It therefore appears that $V_n(I)$ and subsequently $V_n(a)$ is a function of $q(e_n)$. The set of 9 equations can hence be solved only be iteration. The full estimation procedure used in ITER is as follows:

1- estimate $e_n = \frac{1}{2}(n+1)I_n$;

2- ignore second order term of equation (3) and fit the following polynomial[1] in d_n and a_n (3) :

$q(e_n) = A + B\ln(e_n) + C[\ln(e_n)]^2$ (l_n is the Natural Logarithm)

3- use the q(a) fitted in Step 2 in combination with the age of the preceding child, z, and the number of surviving preceding children born z years ago to estimate $V_n(I)$

4- use $V_n(I)$ to compute $V_n(a)$ and apply it to $\frac{1}{2}q''(e_n)$ $q''(e_n)$ is obtained as the second derivative of q in Step 2.

5- recompute $q(e_n) = d_n - \frac{1}{2}q''(e_n)V_n(I)$ and fit the same function as in Step 2.

6- restart from 3 and repeat steps 3 to 5 until convergence. We consider to have reached convergence when an additional round (Step 1 to 5) does not imply a change greater than 0.00001 in either of the estimated $q(e_n)$'s,

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[1]This function was chosen because it fits well the curve representing the cumulative probabilities of dying between ages 1 and 15 in all conventional Model Life Tables that we tried.

with $n = 1,2,...,9$.
With the Bobodioulasso data, convergence is reached after 8 such iterations.

We finally have A, B and C. And q(a) for exact age a is estimated by the formula: $q(a) = A + B\ln(a) + C[\ln(a)]^2$
Therefore, if mortality has remained constant one can solve for the age pattern of mortality.

4.2.2- Age of the preceding child not available: GEOM and MLTA.

The preceding Section shows that the estimation of the full model (ITER) requires a large amount of data and involves complicated estimation procedures. Because the data required by the full model are not always available - mainly the age distribution of preceding children - and for the sake of ease of application, two simplified versions are proposed in this Section. Both models are based on the dropping of the 2nd order term of Equation 3.

At this level, a clear distinction between exposure time and reference age is necessary. Consider the B_n previous births reported by the W_n women who declare having n previous births. The mean exposure time for these children is the average number of years elapsed since their birth. In Equation 3 we have the following equality:

$$d_n = q(e_n)+\tfrac{1}{2}q''(e_n)V_n(a)$$

where e_n represents the mean exposure time. This mean exposure time e_n is in fact the arithmetic mean of the number of years since the birth of previous children (see Equation 4). The second term on the right hand side of this equation is always negative on the relevant range of ages: the function q(a) is concave and therefore q" is negative and $V_n(a)$ is always positive. This implies that d_n is less than $q(e_n)$. d_n is equal to the probability of dying by an age a_n which is lower than e_n. This is because the q(a) function is not linear. If it were linear then we would have q"=0 and $d_n=q(e_n)$. The age a_n at which $d_n=q(a_n)$ given the non-linearity of the q(a) function is what we define as reference age.

After dropping the second order term, the reference age a_n is estimated and the observed d_n is taken as being equal to $q(a_n)$. Then q(a) for exact ages 2, 3, 4, ... is estimated by interpolation. If the crude proportions are not stable, because of errors in the number of ever born and deceased children or small sample size (as is the for EMIS Bobodioulasso), they need to be smoothed. The same polynomial function that is fit in q(a) is used for this purpose. The only difference between the two models (MLTA and GEOM) lies in the procedure used to estimate a_n, the reference age.

GEOM. This model does not assume a life table. The estimation of a_n is simple. Let I_n be the mean length of the birth interval for women reporting n previous births.

The age distribution of previous births would be as follows.

Birth rank	Age
n	$0.8I_n$
n-1	$2I_n$
n-2	$3I_n$
.	.
.	.
1	nI_n

In ITER, the mean exposure time, e_n, is estimated as the arithmetic mean of this age distribution. After dropping the term of Equation (3) containing the variance of the age distribution, one needs to approximate the reference age as closely as possible. The simulation procedure described in Annex III shows that, for a given mortality schedule and birth interval, the geometric mean gives a better approximation of the reference age. This is because, unlike the arithmetic mean, the geometric mean takes into account the non-linearity of the mortality function.

Therefore we can write:

$$a_n = \sqrt[n]{0.8I_n * 2I_n * \ldots * nI_n} = I_n \sqrt[n]{0.8*2*3*..*n} \qquad (6)$$

<u>MLTA</u> is an adaptation of Fargues' approach to the situation where women are grouped by parity. For mothers reporting n previous births, the proportion deceased d_n is equal to:

$$\frac{D_n}{nW_n} = \frac{W_n \delta_n}{nW_n} = \frac{\delta_n}{n}$$

Where δ_n, the mean number of deceased children, is the same as estimated by Fargues (see Section 3.3.2)

$$\delta_n = q(0.8I_n)+q(2I_n)+.....+q(nI_n)$$

The problem is to determine the reference age a_n such that the observed d_n is equal to $q(a_n)$. This is achieved by assuming a model life table and applying the following procedure. We start by estimating δ_n^s and d_n^s that would be observed given the observed age and parity distribution if mortality was the same as the one used as a standard. The estimated value of d_n, d_n^s, is then situated in the set of $q^s(a)$'s of the standard and a_n determined by interpolation.

4.3. Solution under changing mortality

It is clear that "reports of cumulative events provide no direct evidence on trends in mortality or on age pattern of mortality" (Preston, 1985). The set of q(a)s estimated by either of the techniques described above reflects both age pattern of mortality and mortality trends. It is not possible to disentangle these two aspects unless one is willing to assume that mortality follows a prescribed age pattern or able to specify the underlying trends (idem. p.258). In Section 4.2 we have shown that, if mortality has remained constant -- ie the underlying trend is known -- if in addition the age distribution of preceding births is available, then one can solve for the age pattern of mortality using ITER. The proportion deceased reported by a given group of women is equal to the probability of dying -- in the current life table -- from birth to exact age a_n, $q(a_n)$, where a_n is less than the mean number of years elapsed since the birth of these children. When mortality has been changing, the set of $q(a_n)$ estimates do not belong to the same life table. Feeney (1977; 1980) and Palloni (1980) have shown that, if cohort mortality has been falling linearly, then there is a cohort born C years before the survey to which the estimates under the assumption of constancy pertain. Mathematically, this correspondence also holds in the case where parity is used as a grouping criterion. However, as in the case of age grouping, to be able to identify the relevant cohort and subsequently to infer mortality trends, some assumptions about the age pattern of mortality are required. When the age pattern is unknown, we need to assume one. It is also necessary to assume that this age pattern remains the same throughout the period of interest. It is only under these conditions that variations in the probabilities of dying may be attributable to shifts in the mortality functions of the same model (Palloni, 1980). In other words, one needs to assume a one-parameter model life table. The derivation of time locations of estimates, C, follows from MLTA which involves the assumption of a model life table.

Following these assumptions we can write

$q(a,C) = q^s(a)*(K+rC)$ where:

$q(a,C)$ = probability of dying by age a in the life table applicable to the cohort born C years before survey,

$q^s(a)$ = standard $q(a)$ function in the adopted model mortality.

K,r = parameters of the linear cohort mortality decline.

For mothers reporting n previous births we have:

$$d_n = \frac{B_1(K+rI_n)q^s(I_n) + B_2(K+2rI_n)q^s(2I_n) + \ldots\ldots.}{B_1 + B_2 + \ldots\ldots\ldots\ldots + B_n}$$

$$= \frac{\sum B_i(K+riI_n)q^s(iI_n)}{\sum B_i} = \frac{\sum B_i(K+rT)q^s(iI_n)}{\sum B_i}$$

All summations are done over i = 1, 2,..., n.

After simplification of both left and right hand sides we finally have:

$$C = \frac{\sum iI_n q^s(iI_n)}{\sum q^s(iI_n)} \qquad (7)$$

C has the nice property of being independent of the parameters of mortality decline. C can be computed for each parity group. At any time C we have:

$$\text{Logit}[q(a_n,C)] = \alpha(C)+\text{Logit}[q^s(a_n)]$$

where $\text{logit}[q] = 0.5\ l_n[(1-q)/q]$

In MLTA we have $d_n=q(a_n)$. Therefore knowing $q(a_n)$ and $q^s(an)$, we can calculate $\alpha(C)$. We then use the relationship between $q(1)$ and $q^s(1)$ to calculate the infant mortality rate, experienced by the cohort born C years before the survey (because we assume a one parameter model life table, it is also possible to estimate any other value of the life table).

Note the difference with approaches that translate the estimates under the assumption of consistency directly into period estimates (Coale and Trussell, 1977; Sullivan and Udofia, 1979). Strictly speaking the estimates derived from child survivorship data are cohort estimates. During a period of changing mortality a cohort experiences different period-specific mortality rates and the passage from cohort to period measures involves a certain amount of error (Sullivan and Udofia, 1979). However, the procedure is less problematic in that perspective when the infant mortality rate is the index to be estimated. Even with quite rapid mortality change, the infant mortality rate experienced by a given cohort will be similar to the infant mortality rate prevailing in the year of birth of the cohort. That is not the case for q(5) for example. But because mortality patterns differ mostly at the beginning of life, the estimation of q(1) is sensitive to the assumed age pattern of mortality. To compare different populations, it would be more advisable to use q(2) or, better, q(5).

4.4- Estimation of the mean length of the birth interval

Until now we have assumed that for each group of women, the mean length of the birth interval is known. The purpose of this Section is to provide a way of estimating it.

Brass and McCrae (1984) propose to take the mean length of the birth interval as equal to 2.5 for developing countries when it is unknown. This value is very close to the mean for the 4 WFS African countries for which the calculation was done using WFS data (Hobcraft and McDonald, 1984). The average for Kenya, Lesotho, Senegal and Sudan is 2.4 years (average of orders 2 to 8 trimeans). And use of 2.5 would not imply a big error in the estimates. However, as far as low fertility populations are concerned, the value of 2.5 may not be realistic. Widespread use of contraception leads to greater variance in birth intervals. An explicit way of estimating the mean length of the interbirth interval is needed. The solution we propose here is applicable only to conditional samples (none of the women included has stopped childbearing yet and all intervals are closed).

In a stationary population where all women have the same pace of reproduction and start childbearing at the same age, the birth interval will be constant and equal to the difference between the mean ages of women having births of any two successive orders. For example if X_n and X_{n+1} are the mean ages of women reporting respectively n and n+1 previous live births, then $I = X_{n+1} - X_n$. If the pace of reproduction is not homogeneous, then $X_{n+1} - X_n$ will decrease when n increases. This is because women with a slower reproduction will progressively fall behind and those who stopped childbearing will be excluded altogether at high parities.

However, when all women do not start childbearing at the same age as is the case in real life, then this selection effect will be diluted. Many women will be at high parity groups not because of shorter birth intervals but because they started childbearing earlier than average. The dilution of the selection bias will be greater the bigger the variance in the age at onset of childbearing. In real populations the age where childbearing starts is not only variable across women but also it is subject to change. The length of the birth interval for individual women is also known to increase with age and parity. For all these reasons, when one deals with the cross-section of a still reproductive female population, it is not obvious at all that high parity is still synonymous with shorter birth interval. We therefore think that, for all practical purposes, it would be sufficient to estimate the mean birth interval for the whole female population, Ibar, and use that estimate for all parity groups.

Let f(x,n) be the age-specific n^{th} birth rate

$$f(x,n) = \frac{W(x,n-1)}{F_x} \qquad (8)$$

Where W(x,n-1) is the number of women giving birth the year of the survey and who report having n-1 previous births. Fx is the size of the female population aged x.

X_n, the mean age of women having their n^{th} birth is a weighted average of the age-parity specific fertility rates

$$X_n = \frac{\sum xf(x,n)}{\sum f(x,n)} \qquad \text{over } x = o \text{ to } \infty \quad (9)$$

If the age at first birth were the same for all parity groups, then the average birth interval for women having their n^{th} birth, I_{n-1}, would be the difference between the mean age of these women and the mean age of women having their first birth divided by the number of previous births, n=1. We can write

$$I_{n-1} = \frac{X_n - X_{n-1}}{n-1} \qquad n = 2,3,..,10 \qquad (10)$$

Use of Equation 10 is equivalent to using different birth interval estimates for women of each parity [I_n=f(n)]. Application to US data indicates that use of the set of In calculated from Equation 10 - where the birth interval decreases with parity - leads to an overestimation of mortality trends (see next Section). It seems to be more appropriate to use Ibar, the weighted average of the set of I_n for all parity groups.

$$\text{Ibar} = \frac{\sum W_n I_n}{\sum W_n} \qquad n = 1,2,\ldots,9 \qquad (11)$$

Equation 11 applies the same estimate for women of each parity [I_n=Ibar for n=1, 2,..., 9].

4.5- Estimating Infant mortality trends for the US White Population in the Birth Registration Area: 1919-1933.

Since the creation of the Birth Registration Area (BRA) in 1915, questions on children ever born and children surviving have been asked of all American mothers at the time a birth certificate is completed for each new birth. These data were published on a yearly basis by the Bureau of the Census. They offer a good opportunity for testing the method. But we were first reluctant to try it on a low fertility population. Our major concern was about the estimation of Ibar: its estimation from Equation 11 might be very wrong. Another concern was the fact that, between 1915 and 1933, the US was undergoing unprecedented social and economic change which would necessarily be reflected in the fertility behavior of the population (the method assumes constant fertility). In the same time mortality was falling rapidly. However, despite rapid fertility and mortality decline, the technique gives infant mortality trends which are comparable with the ones recorded by the Civil Registration System. The data for three years are used to estimate trends in the IMR: 1919, 1924, and 1933. For these three years, the Bureau of the Census reports give the age and parity distribution of all women reporting a birth (US Bureau of the Census, 1921; 1926; 1936). They also give the distribution of reporting women by parity and the number of their previous births who are still alive. These tables allow us to calculate the proportions dead by parity - the raw material for the method - and to estimate the mean length of the birth interval.

Table 4.1 Selected statistics for the US Birth Registration Area (BRA) in 1919, 1924, and 1933.

	1919	1924	1933
Number of States included	24	34	All US
Tot.Pop.in millions	61.4	86.3	125.7
% of US population	58.6	76.2	100.0
Crude Birth Rate per 1000	22.3	22.4	16.6
Number of White Women giving birth	1,128,963	1,600,091	1,685,001
% of first born*	24.4	29.4	32.5
Mean number of ever born*	3.17	3.13	2.98
Recorded IMR per 1000*	83.0	66.8	52.8
Proportion of previous births who died*	0.1674	0.1449	0.1169
Estimated mean interval**	2.18	2.41	2.36

* White population only
** Estimated using Equation 11

Sources: US Bureau of the Census 1921, 1926, 1936.

The least we can say from Table 4.1 is that the BRA population was undergoing major demographic transformations between 1919 and 1933. During those 15 years, fertility and mortality have declined noticeably. Some caution is however needed to interpret these changes: they reflect two things. First of all they reflect a genuine demographic change which is correlated with social and economic changes. Secondly, changes in the different indices reflect changes in the characteristics of the populations of the different states which are progressively added to the BRA. It is only in 1933 that, for the first time, the BRA covers all Continental US.

The proportions of deceased children reported by white mothers giving birth in 1919, 1924, and 1933 are presented in Figure 4.1. They reflect nicely the mortality decline that occurred between these dates. To estimate infant mortality trends from the observed proportions deceased, the only additional information needed, apart from a model life table, is the mean length of the birth interval. Information on the mean length of the birth interval is provided by the age and parity distributions of women reporting a birth in 1919, 1924, and 1933. The age-parity specific birth rates are first estimated using Equation 8. The denominator in this equation (total female population aged x) is taken from the 1920 Census for 1919 and 1924 and from the 1930 Census for 1933[2] (US Bureau of the Census, 1922 ; 1933). I_n is then estimated from Equation 10 and Ibar estimated using Equation 11. Ibar estimated for the three years are respectively 2.18, 2.41, and 2.36 (Table 4.1). The third model, MLTA assuming the West model life table is used to estimate mortality trends. The infant mortality estimates are given in Table 4.2 and graphed in Figure 4.2 Two cases are distinguished:

a) I_n=Ibar for n= 1,2,...,9 - Panel A of Figure 4.2

b) I_n=f(n): use I_n calculated from Equation 10 - Panel B of Figure 4.2

The major features of Figure 4.2 are:

i) The estimated trends are consistent with the observed IMRs

ii) Estimates based on reports of women who have only one previous live birth (circled in graph) are noticeably higher than other estimates when the same birth interval is used for all parity groups. This tendency reflects the fact that first born have experienced higher death rates than births of higher orders. The excess mortality of first born is exaggerated by the flu epidemic in 1918. Note the similarity with what is generally observed for age group 15-19 in the traditional Brass method (for example see Feeney, 1980)

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[2] Only a rough approximation of the percent age distribution is needed.

iii) When I_n is allowed to vary with parity, [I_n=f(n)]) (Panel B of Figure 4.2) then first born show no excess mortality. This seems however to be spurious. The fact that all estimates are shifted up to become consistent with the exceptional mortality of 1918 suggests that the procedure [I_n=f(n)] overestimates the IMR. Note that the 1919 report imputes 10.5% of the decline in the IMR between 1916 and 1919 to the decrease in the proportion of first born. It is estimated that these first born had a mortality 33% higher than second born (US Bureau of the Census, 1921:20). Unfortunately, no information on the excess mortality of first born relative to all higher order births is not available. A method for accounting for the higher mortality of first born is developed in the following Section.

FIG 4.1: Proportions of Deceased Children by Parity

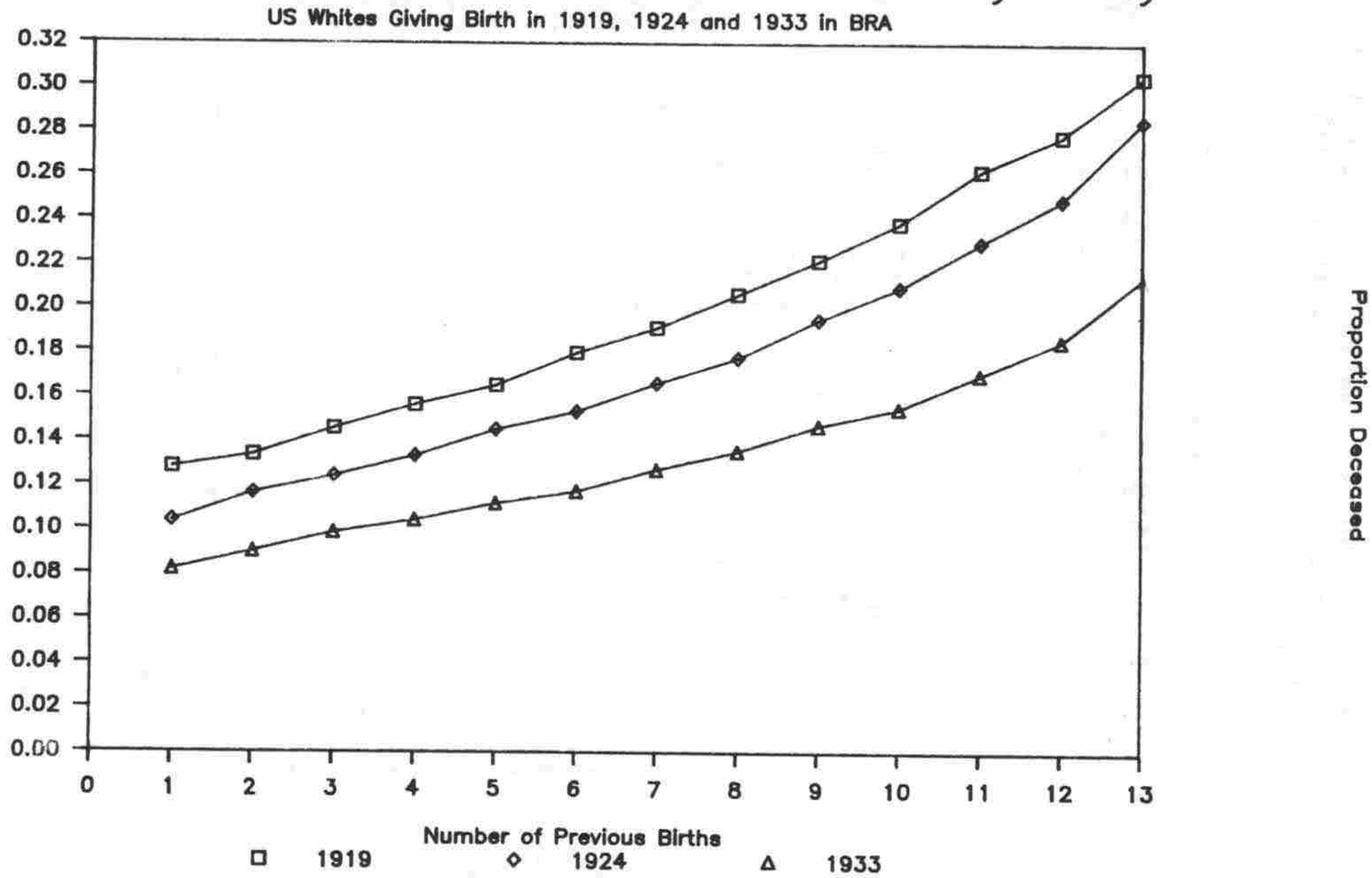
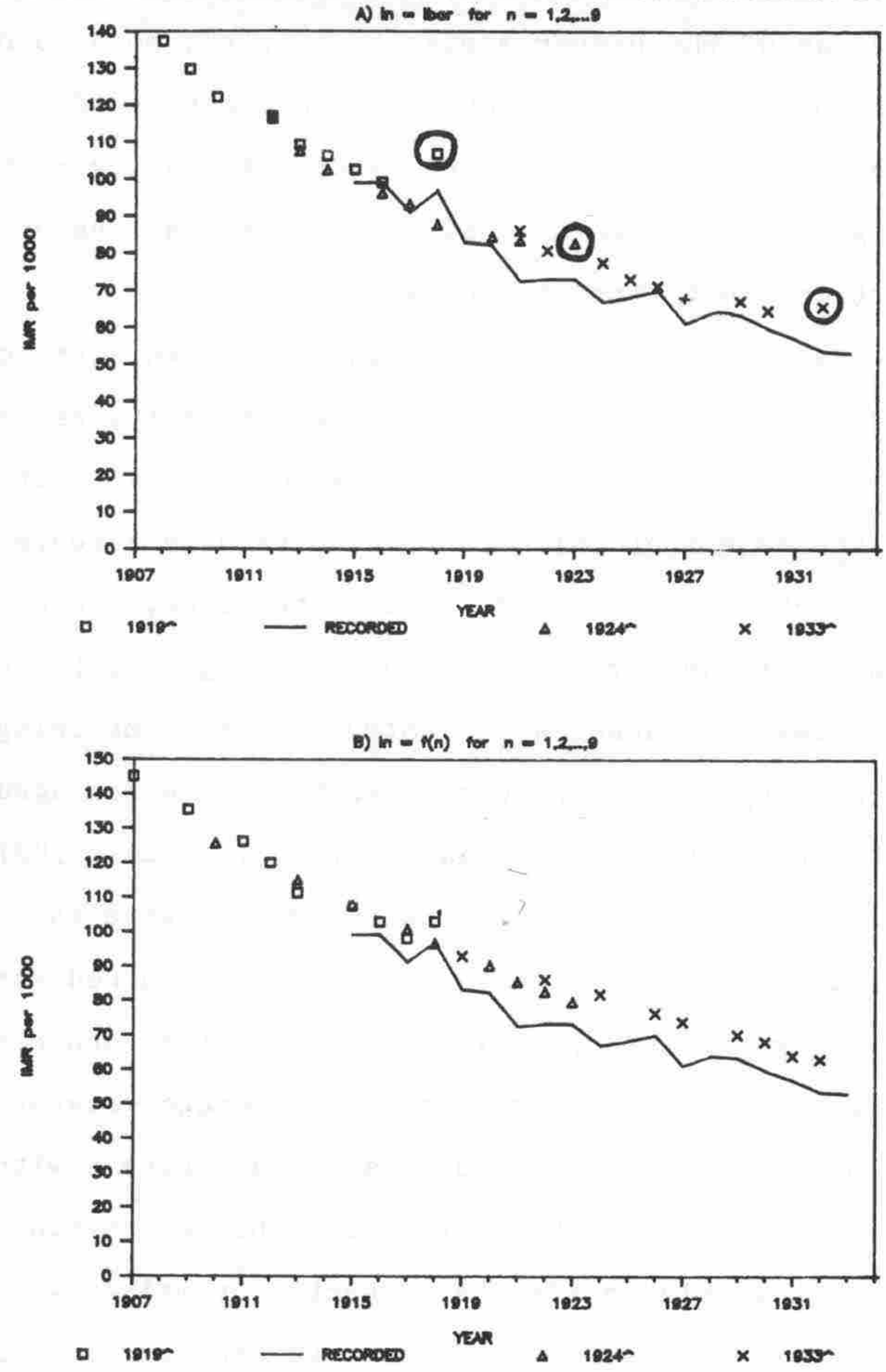


Table 4.2- Recorded and estimated IMR in the Birth Registration Area 1919, 1924, 1933: US white population

		IMR Estimated from Reports in					
	Recorded	1919		1924		1933	
Year	IMR	(a)	(b)	(a)	(b)	(a)	(b)
1907			145.0				
1908		137.3					
1909		129.6	135.4				
1910		121.9			125.6		
1911			126.1				
1912		116.7	119.9	116.2			
1913		109.1	111.2	107.5	114.9		
1914		106.2		102.3			
1915	99.0	102.5	107.3		108.0		
1916	99.0	99.1	102.8	96.2			
1917	91.0		97.9	93.1	100.6		
1918	97.0	106.8	102.9	87.7	96.5		
1919	83.0					92.8	
1920	82.0			84.5	90.0		
1921	72.5			83.5	85.2	86.1	
1922	73.2				82.4	80.7	85.9
1923	73.0			82.7	79.5		
1924	66.8					77.4	81.6
1925	68.3					72.9	
1926	70.9					71.1	76.1
1927	61.0					68.2	73.6
1928	64.0						
1929	63.2					67.0	69.9
1930	59.6					64.4	67.9
1931	56.7					63.8	
1932	53.3					65.5	62.9
1933	52.8						

(a) - estimates based on I_n=Ibar
(b) - estimates based on I_n=f(n)

FIG 4.2: Estimated vs Recorded IMR for Whites in BRA



4.5.1- Accounting for non-homogeneous mortality experience.

Because of the higher mortality of first born, parity groups where these represent a high proportion of all previous births will present an excess mortality relative to other groups. To yield consistent estimates, the crude proportions must first be corrected.

Mortality is non-homogeneous when children born to one or many groups of women have experienced mortality rates which differ non-randomly from the population average. It is often the case that children born to teenage mothers have higher mortality rates and, as a consequence, mortality estimates derived from reports of women aged 15-19 are almost always regarded with distrust. The same thing sometimes applies to children ever born to women aged 20-24 as noted by Feeney(1980), Blacker(1981), Ewbank(1982) and Sullivan and Wilson(1982). One reason for this is that a high proportion of births to these women occurred when they were teenagers. Also, many of them are first order births. First born generally seem to have on average greater chances of dying than higher order births as is apparent with the US data used in this Chapter. Age of mother and birth order have closely related effects on mortality mainly when most first order births are also births to teenagers. Our purpose is neither to try and disentangle these, nor to explain them but merely to take the observed differentials into account in our estimation procedures.

The solution used here is based on the more general approach for correcting Brass-type estimates provided by Ewbank(1982). Let R_1 be the relative infant mortality of first born and R_{2+} the relative mortality of 2nd and higher order births. If P_1 is the proportion of first births in a year, then R_1 and R_{2+} are linked by the following relationship:

$$P_1 R_1 + (1-P_1)R_{2+} = 1$$

When women are regrouped by parity, the relative mortality of children born to either group will be a function of the proportion of these which are first births. For example the proportion deceased reported by women with only one previous birth is not comparable to the proportion reported by women with 5 previous births. The lower n, the higher the proportion of first births, and the higher the excess mortality. For women reporting n previous births, the proportion of first born is 1/n and the proportion of births of higher orders is (n-1)/n. To make them consistent, the reported proportions are divided by K_n such that:

$$K_n = R_1/n + R_{2+}(n-1)/n \qquad (11)$$

The procedure is applied to the 1933 data. Application to the 1933 data should be seen as just an illustration of the method because we do not know the real value of R_1. The value 1.15 used here is just a guess.

4.5.2- Adjusting records for the expansion of the BRA.

Because of the expansion of the BRA, the retrospective estimates based on one year's reports pertain to a population which is different from the one that yields the recorded IMR in all preceding years. For example the 1925 estimate of $_1q_0$ based on reports of women giving birth in 1933 is not directly comparable to the value recorded in 1925 because in that year the BRA does not cover the same states as in 1933. In presence of differential mortality between States, the recorded IMRs must be adjusted to be comparable with estimated ones. For example between 1924 and 1933 the new states added to the BRA have an average higher mortality than the ones that were in the BRA in 1924. To adjust the recorded trends we assume that the relative mortality of states observed in 1933 is the same for all preceding years. Adjustment of the recorded trends is done from 1919 to 1933. These are compared with retrospective estimates based on the 1933 reports in Table 4.3 and Figure 4.3.

Recorded and estimated rates are affected by different types of errors and the comparison should take this into account when possible. By comparing the results presented in Table 4.3 to those in Table 4.2 we can see that the adjustment brings the records closer to the estimates. However, the fact that for all points in time, retrospective estimates are higher than recorded rates suggests that the latter are under reported. It is quite possible that reports of births are more complete than reports of deaths. Because of the cumulation of previous births, retrospective estimates are less sensitive to under reporting of children who died. Figure 4.3 also shows that IMRs estimated from retrospective reports by the last three parity groups diverge noticeably from the recorded ones. The reason for this noticeable divergence is probably the fact that, in a population using widely modern contraceptives, high parity women are selected for relatively higher mortality of their previous births.

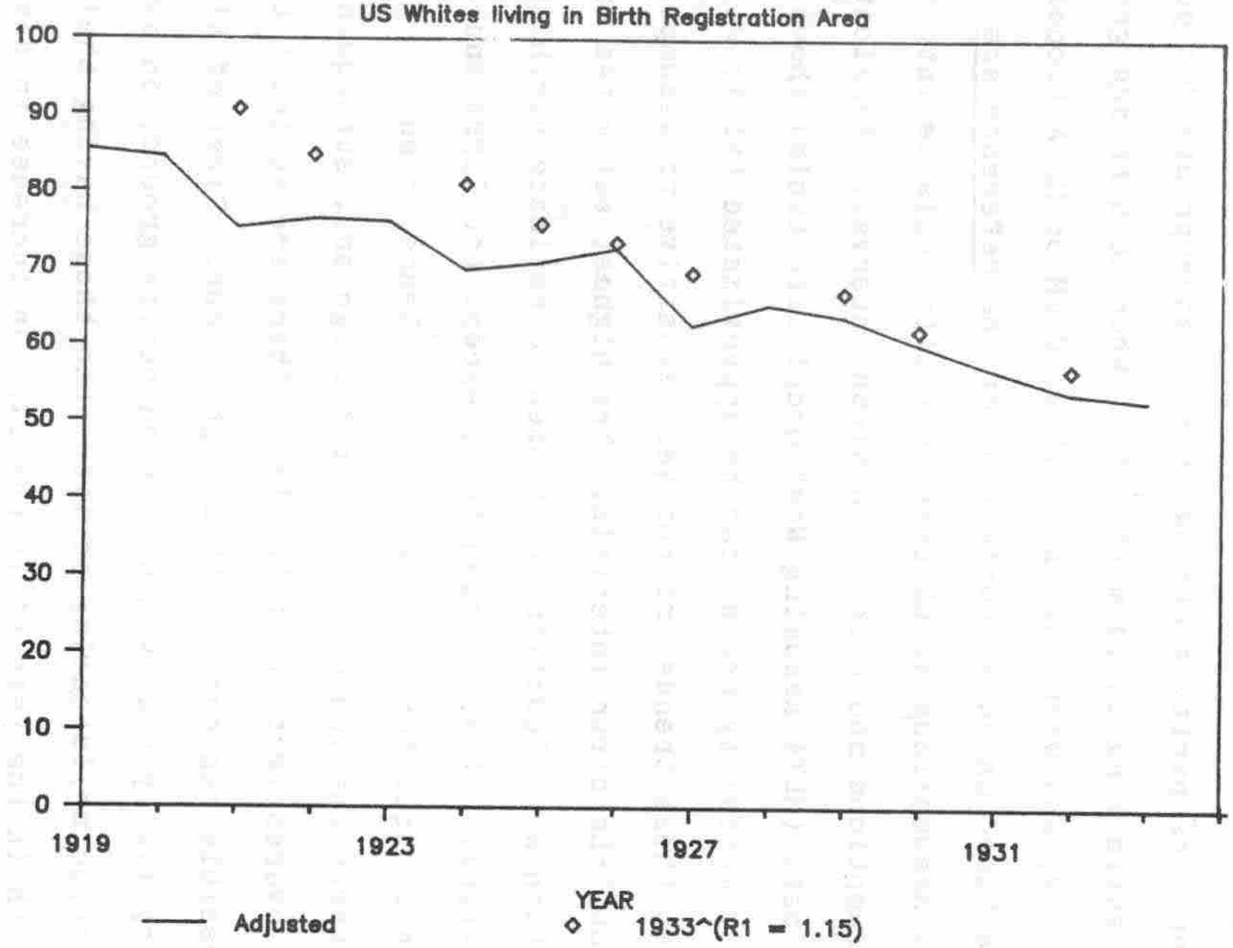
Table 4.3- Observed, adjusted and estimated IMR (US White Population in BRA)

Year	Recorded	Adjusted*	Estimated**
1919	83.0	85.5	
1920	82.0	86.5	
1921	72.5	75.2	90.7
1922	73.2	76.4	84.7
1923	73.0	76.0	
1924	66.8	69.8	80.9
1925	68.3	70.8	75.7
1926	70.0	72.6	73.3
1927	61.0	62.6	69.4
1928	64.0	65.3	
1929	63.2	63.7	66.8
1930	59.6	60.1	61.9
1931	56.7	56.8	
1932	53.3	53.8	56.8
1933	52.8	52.8	

* Records are adjusted for the expansion of the BRA

** Estimates are adjusted for the higher mortality of 1st born, $R_1=1.15$.

FIG 4.3: Estimated versus Adjusted IMR



4.6- **Summary**.

With data from conditional samples, parity gives a better indication of exposure time than mother's age. Therefore parity grouping is more appropriate for purposes of estimating child mortality. When mothers are grouped by parity, and when one uses either GEOM or MLTA (models which are based on an approximation of the reference age, a_n) then all assumptions about past fertility collapse into assumptions about the mean birth interval. Application to US data (MLTA assuming West model life table) shows that past mortality trends can be approximated fairly well and that these trends are not very sensitive to assumptions about the birth intervals. The highest estimates of IMR are gotten when Equation 10 is used to estimate birth intervals: In=f(n). It would seem that, except for first and 2nd born, use of Equation 10 to estimate I_n leads to an underestimation of the reference age and, subsequently, to an overestimation of the IMR. There are at least two possible reasons why this might occur. First of all, it is very likely that women in high parity groups, on average, started childbearing earlier than those having their first birth in the year of interview. An increase in the mean age at first birth would also have a similar effect. However, the estimated trends are still consistent with the observed trend and the ones estimated under the assumption that the same birth interval, Ibar, applies to all parity groups.

CHAPTER 5

CHILD MORTALITY LEVELS AND TRENDS IN BOBODIOULASSO.

5.1- Introduction

In this Chapter, the new methodology is applied to the EMIS Bobodioulasso data. Application to the Bobodioulasso data involves an additional feature of the technique relative to application to US data: the smoothing of the proportions deceased. The sets of q(a) estimates yielded by the 3 versions of the method are compared with those given by the other approaches described in Chapter 3. Finally, infant mortality trends in Bobodioulasso are estimated and a sensitivity analysis conducted.

5.2- Correction of the proportions of deceased children

Because of a relatively small sample size and errors on both the numbers of children ever born and children surviving, the proportions deceased by parity are not stable (see Figure 5.1). Also because of the higher mortality of first born children (see Annex II), parity groups where these represent a high proportion of all previous births will present an excess mortality relative to other groups. To yield consistent estimates, the crude proportions must first be adjusted for non-homogeneous mortality experience, and smoothed to eliminate fluctuations. The procedure described in Section 4.5 is used to account for the excess mortality of first born. R_1 is estimated from the question on the survival of the preceding child. It is the ratio of the proportion deceased among first born to the proportion deceased among births of orders 2 and above. We have:

$$R1 = .1403/.1158 = 1.21$$

This estimate pertains approximately to the first two years of life. It should be noted that it is affected by differences in the length of the first closed birth interval and higher order intervals. However, this correction is better than no correction at all. The set of K_n estimated for Bobodioulasso follows.

n	1	2	3	4	5	6	7	8	9
Kn	1.21	1.07	1.03	1.00	0.99	0.98	0.98	0.97	0.97

After adjustment for non-homogeneity, the proportions are smoothed. The same polynomial that is fitted in q(a) (ITER) is used for this purpose:

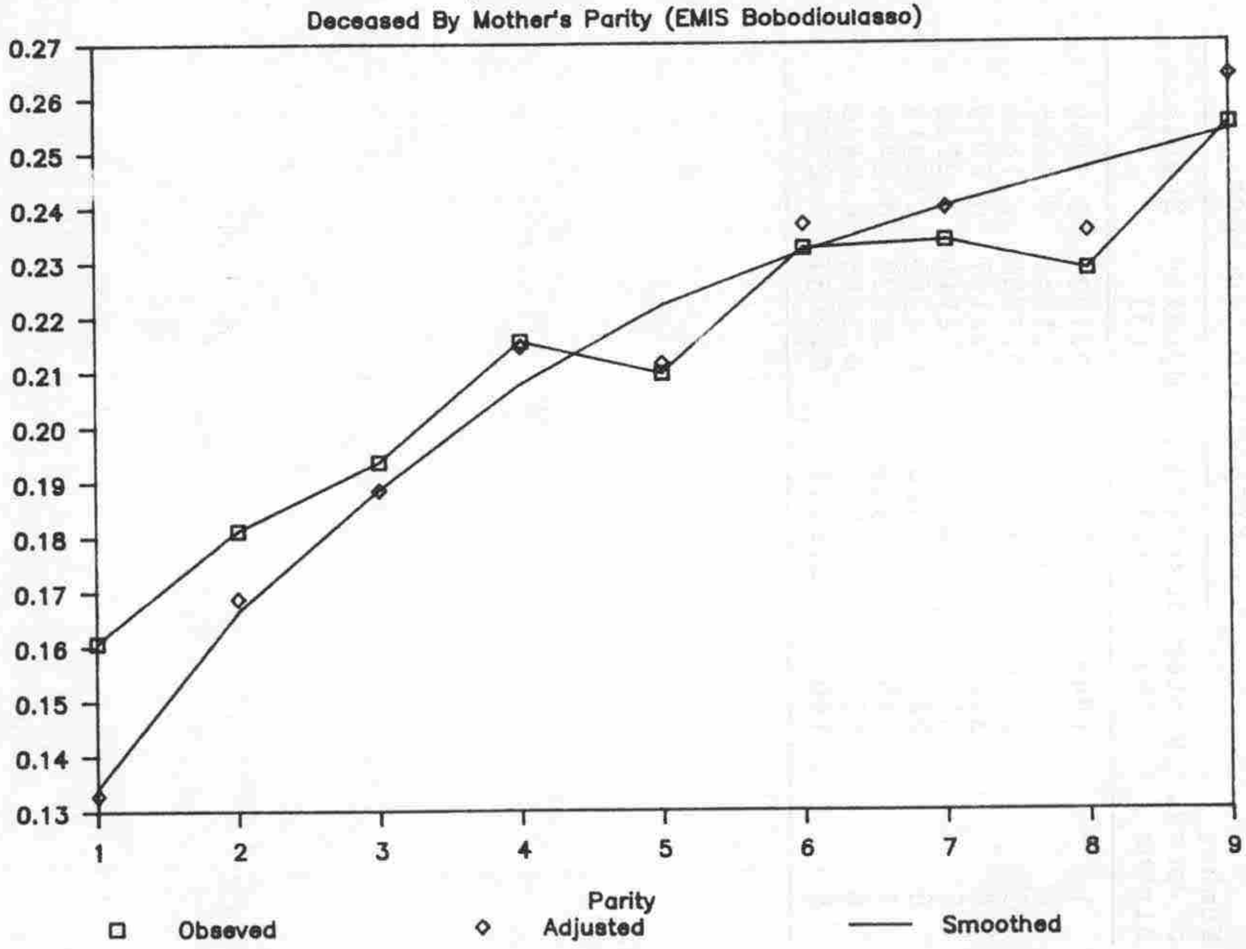
$$d_n = A + B\ln(a_n) + C[\ln(a_n)]^2$$

where ln stands for "Natural Logarithm" (see Section 4.2). Observed, adjusted and smoothed proportions are given in Table 5.1,and graphed in Figure 5.1.

Table 5.1: Proportions deceased by number of previous births reported by the women. (Women giving birth in Bobodioulasso in 1981-82)

Number of prev. births	Number (1)	Proportion deceased Observed (2)	Adjusted (3)	Smoothed (4)
1	1449	0.1608	0.1329	0.1345
2	2318	0.1812	0.1688	0.1665
3	2820	0.1936	0.1884	0.1886
4	3028	0.2157	0.2147	0.2079
5	3185	0.2097	0.2116	0.2222
6	2898	0.2326	0.2369	0.2319
7	2450	0.2339	0.2398	0.2400
8	1960	0.2286	0.2355	0.2470
9	1467	0.2549	0.2637	0.2537

FIG 5.1: Observed, Adjusted and Smoothed Proportions



5.3- Comparison of results: estimates under the assumption of constant mortality.

Table 5.2 presents the results of applying the methodology to EMIS Bobodioulasso. Estimates from other techniques are also presented.

The value of 2.5 years suggested by Brass and McCrae(1984) is assumed for the mean length of the interbirth interval. Application of Equation 11 (see Chapter 4) yields a value of 2.4 years which implies the same estimates both of levels and trends as when the value of 2.5 years is used.

The three models give approximately the same results. ITER gives a slightly higher value for q(2). This is probably because the estimate of q(2) in the other models is based solely on the proportion deceased among preceding children (born to parity one mothers) which may be underestimated as a consequence of under reporting of early child deaths. Errors in the estimation of the variance of the age distribution of previous births, $V_n(a)$, are also an additional source of discrepancy between ITER, on the one hand, and GEOM and MLTA on the other hand. The similarity of estimates given Models G and F is striking: the maximum difference from ages 2 to 10 is 1.4 per 1000 for q(2) and q(3). We presume that, with a better recording of the age of preceding children, ITER estimates would be even closer to those of the other two models. (In addition to the assumptions on birth intervals made by GEOM and MLTA, ITER makes assumptions about the variance of the birth interval distribution based on the age of preceding children which is badly reported.) Therefore, for all practical purposes, either of the simplified versions is an acceptable substitute to the complete one, which is more complicated to implement. A simple pocket calculator may be used to estimate GEOM. The same thing is true for MLTA but, in this case, interpolation between values of the adopted standard is necessary.

As all three models give approximately the same results, let us now compare them with the other methodological approaches.

Table 5.2: Probability of dying by age a under the assumption of constant mortality Application of indirect estimation techniques to EMIS Bobodioulasso.

a	FOLL-UP	APBT	ITER	GEOM	MLTA*	AMFT*	FARG*
1	0.088[a]	-	0.0898	-	-	0.1450	-
2	0.123[a]	0.1327	0.1399	0.1331	0.1345	0.1856	-
3	-	-	0.1689	0.1647	0.1661	0.1949	-
4	-	-	0.1895	0.1866	0.1879	-	0.2089
5	-	-	0.2054	0.2034	0.2045	0.2098	0.2093
6	-	-	0.2183	0.2170	0.2177	-	0.2135
7	-	-	0.2292	0.2284	0.2288	-	0.2183
8	-	-	0.2386	0.2381	0.2382	-	0.2219
9	-	-	0.2469	0.2467	0.2465	-	0.2275
10	-	-	0.2544	0.2543	0.2538	0.2174	-

a: Source: Ouaidou and Van de Walle, 1986.
*: Model life table is African Standard.

Table 5.3: Estimated (MLTA) probability of dying by age a under the assumption of constant mortality. Effect of the assumption that children born to all groups of women have experienced the same death rates (assumption of homogeneous mortality)

Age	Mortality is	
a	Homogeneous	Non-Homogeneous
2	0.1617	0.1345
3	0.1799	0.1661
4	0.1941	0.1879
5	0.2058	0.2045
6	0.2158	0.2177
7	0.2246	0.2288
8	0.2324	0.2382
9	0.2395	0.2465
10	0.2460	0.2538

FIG 5.2: Prop. Dec. by Parity (dn) and by
Age Group (dx) Plotted at Group's Mean Parity (Bobo.)

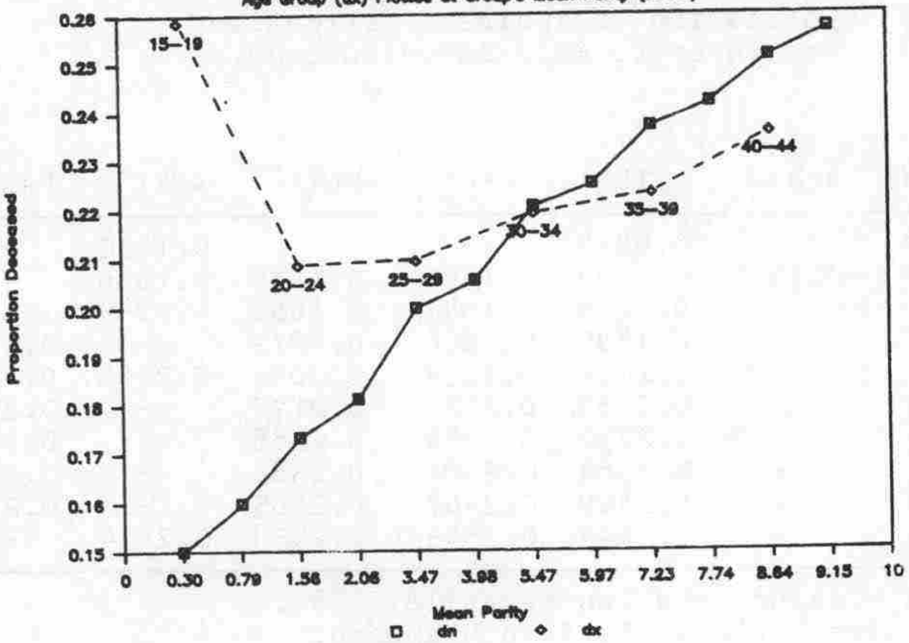
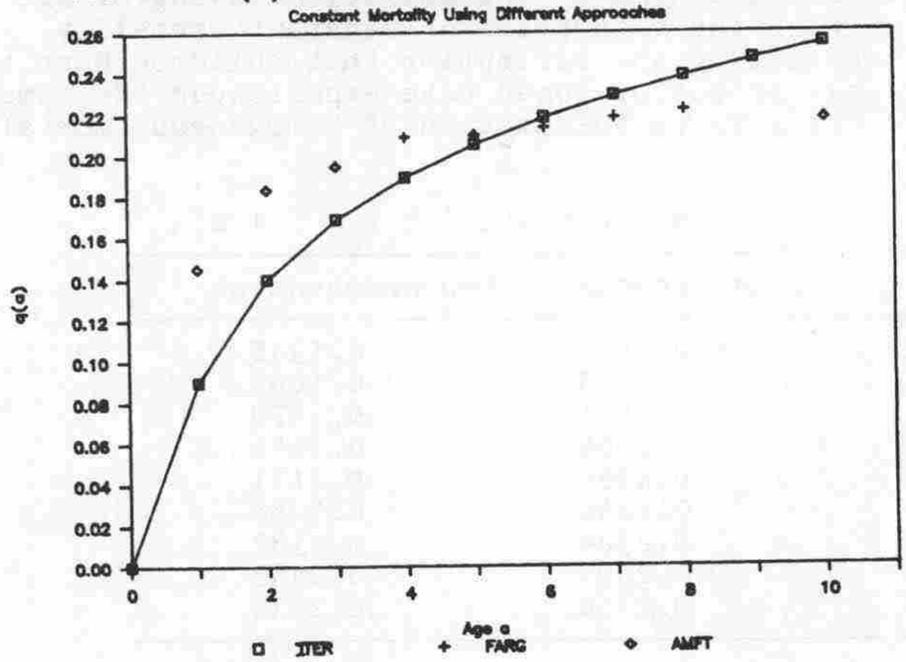


FIG 5.3: Q(a) Estimates Under the Assumption of
Constant Mortality Using Different Approaches



Despite differences in the approaches all techniques give similar values for q(5): between 203 and 210 per 1000 when the African Standard is used by MLTA (see Table 5.2). Compared to the new models, the Adapted Multiplying Factor Technique (AMFT) and Fargues' technique give in general higher estimates before age 5 and lower estimates above that age (see Figure 5.3). Figure 5.2 compares the proportions deceased by age groups, d_x, to the proportions deceased by parity, d_n. The proportions deceased by age group are plotted at the group's mean parity. The proportions deceased for the different mean parities are interpolated between the observed proportions at exact parities 1 through 10. The d_x curve is too flat relative to the d_n curve because of the three factors described in Chapter 1, Section 1.4, which are: reduced variation in exposure, exclusion of previous births to older women who stopped childbearing and selection of younger women who lost their preceding child. Compared to results given by the new approach, the curves representing the sets of q(a) estimates given by the Adapted Multiplying Factor Technique or Fargues' approach are too flat in the image of the proportions deceased by age groups. This result could also be expected partly because these two approaches assume a homogeneous mortality experience. The effect of such an assumption can be seen by comparing the two sets of estimates given by MLTA without and with that assumption Table 5.3. As one could expect MLTA estimates under the assumption of homogeneity are closer to AMFT and Fargues' estimates. The fact that they are still lower under age 5 and higher at older ages is probably a reflection of the fact indicated by our data (Annex II in Chapter 3), that mortality covaries less with parity than with age of the mother.

If we consider the Adapted Preceding Birth Techniquue estimate of q(2) as a reference, then we can say that the new approach performs better than the AMFT and Fargues' technique, at least when applied to EMIS Bobodioulasso. This qualification is necessary because the results produced by all techniques are necessarily dependent on the characteristics of the data set to which they applied. Therefore the new approach needs to be tested on as many other data sets as possible before it can be said to be generalizable.

5.4- **Estimating Infant Mortality Trends in Bobodioulasso.**

Almost all mortality estimates available for wide areas of Burkina Faso come from three major demographic surveys which were conducted respectively in 1960-61, 1969 and 1976. These survey data indicate that Burkina Faso is one of the countries that have the highest mortality levels in Subsaharan African and in the world in general. The World Development Report for 1985 (World Bank, 1985) indicates, an

IMR of 148 per 1000 in 1983. According to the same source, only three countries in Subsaharan Africa have a higher estimate of the IMR: Malawi, Guinea and Sierra Leone. According to UN estimates, from 1960 on, a higher IMR for a country has been recorded only in Sierra Leone in 1970-71 (UN, 1985). Existing data suggest that, at the national level, mortality has declined from a high 263 in 1960-61 (Brass, 1968) to 166 in 1976 (INSD, 1981). It should be noted however, that the two major cities, Ouagadougou (Capital City) and Bobodioulasso which probably have lower mortality were not included in the 1960-61 survey. The World Bank estimates suggest a further decline in 1983 when the IMR is estimated at 148 per 1000.

Three ORSTOM studies have revealed substantial mortality declines in rural Burkina Faso since the 1950's. (Pilon, 1982)[1]. Because of the concentration of most of the economic, educational and medical infrastructures in Ouagadougou and Bobodioulasso, survival gains have, in all likelihood, been more important in these urban centers. It also appears that, on average, mortality levels are lower in Bobodioulasso than in any other locality of the country, the capital city Ouagadougou included (Harrington, 1977;

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[1] The samples for these studies are probably not representative of the rural population from which they were drawn. The ORSTOM studies are based on an exploitation of parish registers which involve exclusively baptized christians which may be very different from the general population. Nevertheless, it may be expected that the estimated trends will mimic trends for the general population

INSD, 1981). It would therefore be unrealistic to assume that mortality has remained constant in Bobodioulasso in the recent past. The estimated infant mortality trends for Bobodioulasso given by the new methodology are presented in Table 5.4 along with estimates from other sources for the country as a whole and for three rural populations studied by Pilon (1982).

Our estimates (in Table 5.4) suggest that the infant mortality rate has remained about at a constant level between 1969 and 1974. Noticeable mortality decline is observed only after 1974. The pattern of decline from 1974 to 1980 is consistent with the estimate of 88 per 1000 for 1983 derived from the EMIS follow-up survey (Ouaidou and van de Walle, 1986). The constancy of the IMR between 1969 and 1974 seems also to be a genuine reflection of one of the most severe droughts in the recent history of the Sahel. The drought started in 1968 when in some areas, rain fell one-fifth below normal and one-third or more below that of the 1950's (Caldwell, 1975). It is only in 1974 that the region had some relief. The countries the most affected by the drought were Mauritania, Mali, Senegal, Chad, Niger, and Burkina Faso (formerly Upper Volta). The trends for the three rural localities also show a slight increase or constancy of the IMR from 1965-69 to 1970-74 (columns 2 to 4). The consistency of the rural trends with our estimates for Bobodioulasso suggests that both rural and urban areas suffered from increases in loss of life during the big Sahelian drought.

5.5- **Sensitivity Analysis**

All the preceding discussion assumed that the different parameters of the model were quantified correctly. Let us now see how variations on parameter estimates will affect the results. (see Table 5.5)

Sensitivity to the estimation of Ibar

The overall pattern of mortality decline is not sensitive at all to the estimated value of Ibar. Even the mortality level is not sensitive to small váriation in Ibar. For example, taking Ibar = 2.5 years instead of the estimated 2.4 years would make no significant difference in the estimated mortality levels. The general tendency is that, the higher Ibar the lower the estimated mortality level, while the pattern of trends remains unaffected.

Sensitivity to the estimation of R_1.

As could be expected only the infant mortality estimates derived from the first two parity groups (n=1 and n=2), and to a lesser extent from the 3rd parity group are affected by variations in the value of the relative mortality of first born, R_1. If for one reason or another it is not possible to estimate R_1, trends estimated from parity groups three and above are still correct.

Sensitivity to the choice of Model Life Table.

To test for sensitivity to the choice of a model life table, results obtained by using the African Standard and the Bandafassi life table are shown in Table 5.5. Use of Brass's African Standard suggests that mortality has remained approximately constant between 1969 and 1974 at about 120 per 1000 live births. After 1974, a rapid decline is noted: the infant mortality rate drops from 119 in 1974 to 96 per 1000 in 1980. When used as a standard, the Bandafassi life table suggests that mortality has been declining slowly but steadily since 1969. It also yields lower levels of IMR in the whole period. According to this model life table, mortality has fallen from 101 in 1969 to 87 in 1980.
Estimates of q(2) and q(5) would be less sensitive to the assumed model life table.

Table 5.4- Infant mortality trends in Burkina Faso (IMR per 1000)

Year	Burkina (1)	Koungoussi Tikare[d] (2)	Reo[d] (3)	Mariatang[d] (4)	Bobo (5)
1950					
1951					
1952		218	164	137	
1953					
1954					
1955					
1956					
1957		164	155	137	
1958					
1959					
1960	263[a]				
1961					
1962		130	127	105	
1963					
1964					
1965					
1966					
1967		99	117	86	
1968					122.1[e]
1969					
1970					122.1[e]
1971					121.7[e]
1972		103	114	102	120.7[e]
1973					
1974					118.9[e]
1975					115.1[e]
1976	166[b]				
1977					109.9[e]
1978					103.8[e]
1979					
1980					95.8[e]
1981					
1982					
1983	148[c]				88.0[f]

Sources: a: Brass (1968) - b: INSD (1981)
c: World Bank (1985) - d: Marc Pilon (1982)
Pilon's estimates are for 5 year periods, they are plotted here at about the mid-period.
e: estimated using MLTA assuming African Standard
f: estimated from EMIS Follow-up Survey (Ouaidou and van de Walle, 1986)

Table 5.5: Infant Mortality Trends as a function of variations in the parameter estimates for Emis Bobodioulasso (IMR per 1000).

Year	Value of Ibar				Value of R_1		Model Life Table	
	1.8	2.2	2.6	3.0	1.21	1.0	African	Bandaf.
1966				117.4				
1967				115.6				
1968			122.0		122.1	117.7	122.1	
1969			120.2	113.3				101.0
1970		127.0			122.1	118.0	122.1	100.2
1971		127.0	120.0	113.5	121.7	118.1	121.7	98.9
1972	134.3	126.5	119.2	113.0	120.7	117.9	120.7	
1973	134.1							96.2
1974	133.2	125.4	117.6	111.3	118.9	117.1	118.9	92.6
1975	131.2	122.8	114.2		115.1	115.4	115.1	91.3
1976	129.2	119.3	109.0	106.4				
1977	126.8	115.0		99.8	109.9	113.7	109.9	90.4
1978	123.6	110.2	102.9		103.8	113.0	103.8	88.9
1979	118.9			89.2				
1980	114.0	102.8	94.6		95.8	116.1	95.8	87.3

Note: For Bobodioulasso, the assumed values for Ibar and R_1 are respectively 2.5 and 1.21 and the selected model life table is the African Standard. Whenever a value is not stated for a given parameter the following values are implicitly assumed: Ibar=2.5, R_1=1.21 and Model Life Table=African Standard.

5.6- Summary

In this chapter the new methodology is used to estimate infant mortality trends in Bobodioulasso. We have first shown that, if the Adapted Preceding Birth Technique estimate of q(2) is taken as a reference, then one conclude that the new method performs better than other existing approaches. The set of q(a) estimates produced by the Adapted Multiplying Factor Technique and Fargues' approach are too flat in the image of the proportions deceased by age group on which they are based. The estimation of trends using the African Standard suggests that mortality has remained about constant during the heavy drought that struck the Sahel from 1969 to 1974. From a value of 119 per 1000 in 1974 the infant mortality rate fell quite rapidly afterwards, to reach the value of 96 per 1000 in 1980.

When used as a standard, the Bandafassi life table shows a steady but very slow mortality decline between 1969 and 1980. The infant mortality rate goes from 101 to 87 per 1000. The implied conclusion is that mortality decline in Bobodioulasso appears to have been, on average, relatively slow during the 10 years preceding the EMIS survey. One major weakness of the new technique and of all indirect techniques in general is that, in most cases where they may be useful, the correct age pattern of mortality is not known. For this reason, when the objective is to compare different populations, it would be more appropriate to estimate trends in q(5) or q(2) which are less sensitive to the assumed model life table.

Estimating trends in the infant mortality rate, we had the presentiment that it would be possible to use the follow-up IMR to evaluate the consistency of evaluate the results. But that is not really the case. Despite the fact that the value of 88 per 1000 (in 1983) estimated by Oua dou and van de Walle (1986) is consistent with the estimated trend (119 in 1969 and 96 in 1980), it cannot be used as a reliable test for the validity of retrospective estimates. The follow-up estimate is itself affected by two factors with contradictory effects. The cohort of <u>current</u> births experienced a measles epidemic in 1982 (van de Walle, 1985). This tends to pull the the 1983 estimate above the normal level of infant mortality if measles epidemics do not occur on a yearly basis. The opposite factor is the immportance of losses to follow-up (about 19% of the initial sample). As EMIS Bobodioulasso and probably all EMIS are more likely to lose higher mortality women, this factor will tend to an underestimation of current mortality. It is not possible to tell the relative importance of both factors to be able to estimate the resulting bias.

CHAPTER 6

SUMMARY AND CONCLUSIONS

6.1- Introduction

The objective of this research was to develop a methodology for deriving mortality estimates from the retrospective child survivorship data collected through the EMIS surveys (Infant Mortality Surveys in the Sahel). The retrospective analysis is complicated by the fact that the data made available by the EMIS are not commonly used in Demography. Retrospective studies of fertility and mortality are usually based on reports from a random sample of women representing a cross-section of the female population at a given time. The EMIS samples are of a different type. They comprise women giving birth within a definite period and therefore include women who are not only still fecund, but have given proof of their fecundity during the reference period. This type sample is referred to as conditional (inclusion is conditioned by delivery within the reference period) to distinguish it from the usual random sample which represents the unconditional case. In addition to including women whose characteristics are different from those included in the unconditional samples, the EMIS data differ from traditional retrospective data because of the timing of the interviews: all women are interviewed at the time of birth of a child. The first implication of this unusual timing is that the previous births are on average older than those represented in the unconditional case. As a consequence, the traditional Brass technique (Brass and Coale, 1968) for estimating mortality from child survivorship data is not applicable. Different types of data call for different tools of analysis.

6.2- Techniques for mortality estimation applicable to conditional samples.

Brass and McCrae (1984, 1985) propose two ways of estimating mortality applicable with data from conditional samples. These are: the Preceding Birth Technique (PBT) and the Adapted Multiplying factor technique (AMFT). The Preceding Birth Technique requires that one simple question be asked of women giving birth: "Is your preceding child alive?" In populations where the mean length of the birth interval is close to 2.5 years, the proportion of last births who died is shown to be a robust estimate of q(2), the probability of dying from birth to exact age 2 years (see Chapter 3). Because the question asked by the EMIS surveys is slightly different from the required one the technique is not directly applicable to EMIS data. It can however be adapted. The adaptation proposed in Chapter 3 yields a value for q(2) of 133 per 1000 approximately in 1980.

Like the original Brass technique, the Adapted Multiplying Factor Technique requires knowledge of age, number of children ever born, and number of surviving children for each respondent. The technique is based on a hypothetical reconstruction of the unconditional from the conditional proportions of deceased children. Application of the Adapted Multiplying Factor Technique to the Bobodioulasso data yields unlikely results, except for the estimate of q(5) which is the same as those derived from the other techniques. The estimated q(2) is equal to 186 per 1000, which is out of the possible range of values suggested by the Preceding Birth Technique (see Chapter 3).

Fargues (1985a) realizes that, when women are interviewed at the time of childbirth, the duration of exposure of previous births to the risk of mortality is a direct function of the mean length of the interbirth interval. He uses this knowledge to estimate the reference age (age at which the proportion deceased is equal to q(a)) for each age group of women and just takes the proportion deceased for each age group as the probability of dying by an age equal to the estimated reference age. Fargues' technique give the same estimate of q(5) as the Adapted Multiplying Factor Technique, does a better job than the latter above age 5, but does not "distinguish itself" at early ages. This is because, like the AMFT, the approach does not provide for the fact that, in Bobodioulasso, children born to women 15-19 and 20-24 have higher mortality. Even if mortality were not a function of mother's age per-se, estimates based on age grouping should be corrected to account for the fact noted in Chapter 1 that young women who are include in conditional samples are those who are having their second or third birth. This is more common among those who lost their preceding child who are therefore a selected group. The general procedure provided by Ewbank (1982) may be used to correct estimates based on age grouping.

Fargues ignores the effect that changing mortality will have on his estimates and does not mention any way of estimating trends. Brass and McCrae (1985) use the single estimate of q(2) given by age group 20-24 in successive years to estimate mortality trends in the Solomon Islands. That procedure is not possible with the EMIS data: they constitute a single point observation. A technique allowing the estimation of mortality trends applicable to EMIS surveys is therefore needed.

Chapters 4 and 5 of this research demonstrate that the maternity history data collected at time of birth of a child may, like conventional retrospective data, be used to estimate recent trends in child mortality from just one year's observation. The major features of the new technique are presented in the next Section.

6.3- An alternative approach: regroup mothers by parity instead of by age.

The approach advocated in this study differs from the preceding ones mainly in the adopted grouping criterion: mothers are grouped by parity instead of by age. The idea of regrouping women by parity for the estimation of mortality is not new. It was suggested by Preston and Palloni (1978) for unconditional data. However, until now only age and marital duration have been used as grouping criteria. Probably the major issue in deciding the choice of a grouping criterion is how well it controls for exposure time, the duration over which previous births have been exposed to the risk of mortality, to be estimated. In the analysis of data from unconditional samples, age and marital duration have proved their worth. For conditional samples, parity is probably a more appropriate grouping criterion. For each parity group, the range of variation of exposure time is narrowed down to the range of variation of the interbirth interval. The age or duration of exposure of previous births to the risk of mortality is a direct function of the mean length of the birth interval.

Three versions of the new approach are presented. The full version, ITER, uses the age distribution of preceding children and is based on an iteration procedure that necessitates the use of a computer. ITER gives life table estimates without the need of a model life table. The second version, GEOM, is simpler and may be estimated using a simple pocket calculator. Like ITER, GEOM does not require the assumption of a model life table. The procedure for estimating mortality trends described in Chapter 4 is derived from the third model, MLTA, which is based on the assumption of a model life table. All three models give similar q(a) estimates when mortality is assumed to have been constant and the African standard is assumed by MLTA. When mortality has been changing as was the case in the US between 1915 and 1933 and in Bobodioulasso the set of q(a) estimates confuses the effect of the age pattern of mortality with those of mortality trends. A guess of the underlying age pattern of mortality is therefore necessary for one to be able to infer the trends.

When the appropriate model life table is known one can use it in Model F to infer mortality trends. The prerequisite for that is to estimate the time location of the estimates under the assumption of constant mortality. The time location of the estimates, C, is a direct function of the birth interval (Equation 7). In chapter 4 the above described approach is applied to US data in 1919, 1924, and 1933. The infant mortality trends estimates from these three observations overlap nicely and are similar to the trends recorded in the US Birth Registration Area. Application to EMIS Bobodioulasso is rendered difficult by the fact that the underlying age pattern of mortality is unknown. Choice of the African Standard yields trends which suggest that, during the severe Sahelian drought of 1969-1974, the infant mortality rate in Bobodioulasso has remained approximately constant. Three ORSTOM studies in Rural Burkina Faso have also shown constancy or a slight increase in the infant mortality rate between 1964-69 and 1970-74 (Pilon, 1982). We have no other means of evaluating the estimates for Bobodioulasso because of lack of data. Application of the methodology to EMIS Bobodioulasso data suggest that the infant mortality rate in Bobodioulasso in 1980 is similar to the one experienced by the US white population living in the Birth Registration Area in 1918: about 96 per 1000 live births. The comparison becomes more striking when one is aware of the fact that the existing evidence indicates that Bobodioulasso has the lowest level of mortality in Burkina Faso.

6.4- Limitations and implications for future research.

Basically the indirect estimation of mortality consist in translating proportions deceased among previous births to different subgroups of women into probabilities of dying in the life table(s) applicable to the children (convert d's into q's). To achieve this, the demographer needs an estimation of exposure time. The estimation of exposure time is achieved in different ways. The original Brass technique assumes a fertility pattern and an age at onset of childbearing (a_0), assumes that the age pattern of fertility and a_0 are constant over time and uses the difference between the current age of the woman and a_0 to estimate exposure time. The version of the technique using marital duration is based on the same principle. Preston and Palloni 1978) use the age distribution of surviving children to estimate the number of years ago since the birth of previous children. With data from conditional samples the mean length of the birth interval becomes a better unit of measurement of exposure time. It is used by Brass and McCrae (1984) in the Preceding Birth Technique and Fargues (1985a). If parity is used as a grouping criterion, as is the case in the approach developed by this research, then all assumptions about fertility collapse into assumptions about the mean length of the birth interval. This research proposes a way of estimating the mean length of the birth interval from the age and parity distribution of reporting women (Chapter 4). Application to the Bobodioulasso data yields a value of 2.4 years. Use of the value of 2.5 years suggested by Brass and McCrae for population with low use of fertility limitation methods seems therefore to be appropriate for Bobodioulasso. However, that may not necessarily be the case for other populations for which the method may be useful. The estimation of the length of the birth interval may be a critical issue. We presume that the approach is sensitive to errors in the age and parity distribution. We would therefore recommend that more effort be devoted to the recording of the age of the preceding child. For a women giving birth, the age of the preceding child is equal to the length of the last closed interval. This information can be used to improve the estimation of the mean length of the birth interval, Ibar. As all available conditional samples data are gathered through the health and civil registrations systems, most of preceding children probably do have a birth certificate. Even if some don't, the fact that the data collector is aware of the need for a good estimation of the age of the preceding child would probably make a difference. Special effort should be devoted to the determination of the date of birth of deceased children to avoid the deplorable fact observed in Bobodioulasso where we have only the age of surviving children. It should be noted that a better recording of age of the preceding child would also allow a better estimation of:

a) the relative mortality of first born, another parameter of the new model;

b) one version of the new approach, ITER, where the variance of the age distribution of preceding children is an important parameter.

It is, however, encouraging that the estimated pattern of mortality trends is not at all sensitive to the length of the birth interval and that levels are not sensitive to small variations in the length of the interval. Using the estimated value of 2.4 for Bobodioulasso, or 2.5 or 2.6 years does not make a significant difference in the mortality level estimated for successive years. Use of the values of 2 or 3 years produces noticeably different levels but the pattern of trends is the same.

More probing for the number of previous births who died for younger women and mainly those who report the current birth as being their first birth is also advisable. We suspect that some women report the current birth as being their first birth if the preceding one died. One can expect that a widespread omission of first born who died will lead to a serious underestimation of the mortality level derived from the first parity group. Estimates from higher parity groups would not be seriously affected by such misreporting but they may suffer from a different type of bias depending on the type of population one is dealing with. As appears to be the case with the US data used is Chapter 4, in low fertility populations, high parity women may be very different from the others. Estimates based on reports of women in the last three parity groups, for example, may be too high.

The estimation of the relative mortality of first born proposed for Bobodioulasso is not accurate. In fact we estimate R_1 as being the ratio of the proportion deceased among first order preceding births to the proportion deceased among all preceding births. If the first closed interval is very different from the average for all orders, then our estimate of R_1 will be biased. However, for high fertility populations, the length of the closed birth intervals do not vary much with parity (Hobcraft et al., 1985).

6.5- Toward improved methods for collecting demographic data in Sub-Saharan Africa

Use of data from conditional samples is not widespread in Demography. However, this does not mean that they constitute a new type of data. Since 1915, for example, questions on the survivorship of previous live births have been asked of all American mothers at the time a birth certificate is completed for each new birth. Availability of this type of data is also probably more widespread than is now apparent. Maternity clinics in many African countries have included in their routine questions to women who come to seek services information on the survivorship of previous pregnancies. We are certain that these data are available at least in Abidjan (Fargues, 1985b), in Bobodioulasso (see Chapter 2 of this dissertation) ant that they are being collected in Bamako (Hill et al., 1985). We have also shown in Chapter 2 that the Hospital records are of comparable quality with EMIS data at least in

Bobodioulasso and that their substitution to the EMIS data would have yielded similar mortality estimates. If these data have not been used often to estimate mortality it is probably because the techniques for doing so simply did not exist until very recently. We share with Brass and McCrae (1984, 1985) and Fargues (1985a) the feeling that now that the techniques do exist, demographers should increasingly use such data for the study of mortality in developing countries in general, and in Sub-Saharan Africa in particular, where the need for reliable data is more acute than anywhere else. Adoption of the approach will be advantageous for at least one reason, it will reduce drastically the cost of collecting data for the study of mortality. The existing administrative circuits - civil registration and health care systems may be used for monitoring mortality trends with little additional cost. The likelihood that the territorial coverage of these services will increase in the future gives more interest to the idea. Brass and McCrae (1984) show that inclusion of the simple question: "is your preceding child still alive?" in the birth registration form allows an easy and robust estimation of q(2). Hill et al. (1985) propose an additional question on the survivorship of the child born immediately before the preceding one. This additional question is expected to give a good estimate of q(5). However, because the number of questions that can be added without rendering heavy the routine questionnaire of the health and civil registration systems is limited, this approach cannot make demographic surveys for the study of mortality obsolete. An instructive study of mortality differentials necessitates, for example, more questions on the characteristics of the respondent and her household than can be included in the routine questionnaire.

Data from conditional samples are not suited for the study of mortality exclusively. El Badry (1967) showed that they are well suited for the study of fertility differentials. They can be also used to estimate age, parity and other group-specific fertility rates. For example, if the coverage of women of age x giving birth in a year is complete and if the size of the female population aged x is known, the age specific fertility rate for age x is simply the ratio of the former to the latter. Increased use of data from conditional samples will in all likelihood help overcome, in a relatively cheap way, the paucity of demographic statistics in Sub-Saharan Africa.

Annex I: Application of the Adapted Multiplying Factor Technique to Emis Bobodioulasso.

Women's Age Group x	Number of women (B)	Number previous births (PB)	(PB+0.5B)	(PB+0.2B)	Number deceased Dx	Adjusted Proportion deceased dcx	Age a	q(a)
15-19	1879	557	1510	939	144	.1533	1	.1450
20-24	2418	3779	4988	6263	788	.1849	2	.1836
25-29	2090	7272	8317	7690	1523	.1980	3	.1949
30-34	1010	5520	6025	5722	1209	.2113	5	.2098
35-39	593	4290	4587	4409	957	.2171	10	.2174
60-66	171	1478	1564	1512	348	.2301	15	.2249
65-69	22	183	194	187	67	.2508	90	.2446

Annex II: Number of preceding births (B) and Proportions deceased (d) by order of birth and age of the mother - EMIS Bobodioulasso.

MOTHER'S AGE GROUP

	< 20		20-24		25-29		30-34		35-39		40 +		ALL AGES	
BIRTH ORDER	B	d	B	d	B	d	B	d	B	d	B	d	B	d
1	364	0.190	845	0.128	138	0.087	-	-	-	-	-	-	1361	0.140
2	72	0.125	652	0.121	330	0.118	40	0.125	-	-	-	-	1102	0.120
3	-	-	308	0.127	504	0.091	60	0.183	-	-	-	-	900	0.111
4	-	-	87	0.103	457	0.109	145	0.097	-	-	-	-	722	0.103
5	-	-	-	-	301	0.106	203	0.099	59	0.102	-	-	609	0.115
6	-	-	-	-	131	0.107	218	0.073	87	0.092	-	-	461	0.089
7	-	-	-	-	44	0.068	144	0.083	120	0.067	-	-	336	0.071
8	-		-	-	-	-	71	0.099	110	0.082	-	-	233	0.107
9	-	-	-	-	-	-	-	-	84	0.119	-	-	154	0.104
10+	-	-	-	-	-	-	-	-	61	0.082	73	0.164	162	0.130
ALL	481	0.186	1934	0.128	1935	0.105	947	0.096	558	0.084	166	0.117	6043	0.116

Annex III: Simulation of the mean age of previous births.

Consider children born to women reporting n previous births. The last child (preceding the one that is just born) was born, on average, I years ago, if I is the mean birth interval. The average duration of exposure to the risk of mortality, mean exposure time, e, is the average number of years since the births of these children. In fact e is nothing but the arithmetic mean age of the children ever born, a_m. But because of the non-linearity of the lx function, the proportion of previous births who died is equal to $q(a^*)$ such that a^*, the reference age, is less than $e = a_m$ (see Section 4.2.2). In fact that is why, in the Preceding Birth Technique, is equal to 0.8*I instead of q(I) (Brass and McCrae, 1984).

The simulation procedure hereafter described shows that the geometric mean age, a_g, is an acceptable approximation of the reference age, a^*. The biggest difference between a^* and a_g is about 6%. The simulation was done in the following way:

i) for a given value of I and number of previous births, we calculate a_m and a_g;

ii) then, assuming a model life table, we can estimate the hypothetical proportion surviving that would have been observed if the assumed mortality function prevailed;

iii) finally the reference age, a^*, is determined as the age at which the observed proportion surviving is equal to la*. The true q(a*) is then equal to 1-la*.

This was done for Brass's African Standard and two logit transformations of the same life table with $\beta = 0.5$ and $\beta = 1.5$.

Because the geometric mean slightly underestimates or overestimates a*, we tried a way of using a combination of a_g and a_m to improve the estimation of a. Assuming that, on the relevant range of ages, the cumulative probability of dying can be approximated by the following function:

$$q(a) = A+Ba+C\ln(a) \qquad (III,1)$$

then the true q(a) is equal to:

$$q(a) = (1/n)\{A+B(a_1+a_2+...+a_n)+C[\ln(a)+\ln(a_2)+...+\ln(a_n)$$

$$= A+Ba_m+C\ln(a_g)$$

$$=q(a_g)+B(a_m-a_g) \qquad (III,2)$$

B is a function of the age pattern of mortality. It is determined for each model life table by fitting the function (III,1) in the q(a) function between ages 2 and 20.

The results are presented in the following Table. They show that this approximation does a better job than when q(a) is taken to be equal to $q(a_g)$ in 5 of the 12 examples presented in the Table. However, the improvement in all those cases is negligible. The approximation of the reference age by the geometric mean age is therefore satisfactory for practical purposes.

	I = 2, n = 3	I = 2, n = 6	I = 3, n = 3	I = 3, n = 6
African Standard transformed by ß=.5				
a_m	4.00	7.00	6.00	10.50
a_g	3.63	5.99	5.45	8.98
a	3.46	6.01	5.48	8.79
$(a_m-a)/a$	0.16	0.16	0.09	0.19
$(a_g-a)/a$	0.05	-0.00	-0.01	0.02
True q(a)	0.3306	0.3472	0.3448	0.3601
$q(a_m)$	0.3358	0.3518	0.3472	0.3667
$q(a_g)$	0.3323	0.3471	0.3447	0.3610
B =	-0.000158	-0.000158	-0.000158	-0.000158
Est. q(a)	0.3323	0.3470	0.3446	0.3608
African Standard				
a_m	4.00	7.00	6.00	10.50
a_g	3.63	5.99	5.45	8.98
a	3.51	6.18	5.56	8.92
$(a_m-a)/a$	0.14	0.13	0.08	0.18
$(a_g-a)/a$	0.03	-0.03	-0.02	0.01
True q(a)	0.1969	0.2217	0.2175	0.2415
$q(a_m)$	0.2036	0.2275	0.2205	0.2512
$q(a_g)$	0.1986	0.2204	0.2168	0.2420
B	0.000544	0.000544	0.000544	0.000544
Est. q(a)	0.1988	0.2209	0.2171	0.2428
African Standard transformed by ß=.5				
a_m	4.00	7.00	6.00	10.50
a_g	3.63	5.99	5.45	8.98
a	3.57	6.38	5.67	9.08
$(a_m-a)/a$	0.12	0.10	0.06	0.16
$(a_g-a)/a$	0.02	-0.06	-0.04	-0.01
True q(a)	0.1091	0.1334	0.1285	0.1536
$q(a_m)$	0.1145	0.1378	0.1308	0.1627
$q(a_g)$	0.1099	0.1307	0.1271	0.1528
B	0.001629	0.001629	0.001629	0.001629
Est. q(a)	0.1105	0.1323	0.1280	0.1553

a_m = Arithmetic mean number of years since birth
a_g = Geometric mean number of years since birth
a = Reference age: age at which the proportion surviving is equal to lx.
Est. $q(a) = q(a_g)+B(a_m-a_g)$

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